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A  
L E T T E R  
To ----- Esquire,

In ANSWER to  
Mr. ROBINSON's Full Confutation  
O F T H E  
Reply to his Remarks  
O N T H E  
E S S A Y

Upon distinct and indistinct VISION.

---

By JAMES JURIN, M. D.  
Fellow of the College of Physicians, and of the  
Royal Society.

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*Genus est hominum, qui se primos omnium rerum pu-  
tant; Nec sunt.*

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L O N D O N:

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M D C C X L I.







To - - - - - Esquire.

S I R,



S the principal intent of the papers I now present you with, is to clear up those points of Science, which have been controverted between Mr. *Robins* and me; I cannot think it will be necessary to enter into any very particular consideration of the *Preface*, which this gentleman-like writer has been pleased to prefix to his *Full Confutation*. For, besides that Science is no way interested in that *Preface*, and that it is written in a style, wherein I am not at all a match for this author; I trouble not my head either with the *surprising modesty* of Mr. *Robins*, who puts his name to what no body else would own upon the rack, or the more surprising *vanity* of *Philaethes*, who wears a *vizard* to avoid being known; I shall neither make it my business to vindicate a Gentleman, who wants none of my assistance, nor can I think, that you, who have so carefully perused the whole controversy between those two writers, and are so good a judge of all that has passed in it, whether in prose or verse, will be at all influenced by such vindication.

But you will not, I am sure, think it improper, that I should endeavour to remove some imputations, and particularly a very heavy charge of immorality, with which though *Philaletbes* only has been directly loaded, yet Mr. *Robins* pretends to fix it upon me, only because I do not give him a Satisfaction he has no right to demand, and, I think, but little reason to expect. For, if what this writer says be true, that *I suffer my self to be universally thought the author of all the scurrilities of Philaletbes, without so much as undeceiving even my friends*, which by the bye is more than he knows, why should he take it ill that he is not undeceived? I have not the honour to number Mr. *Robins* among my friends.

The charge I speak of is, that *Philaletbes* has unjustly \* *accused a Gentleman well known to him, who had never given him any subject of offence, of a crime little short of forgery*. Now, if I am meant by *Philaletbes*, I acknowledge, that Dr. *Pemberton* is a Gentleman well known to me, and within these five years last past better known than ever. But whether he *had never given Philaletbes any subject of offence*, those are the best Judges, who have been witnesses to that Gentleman's daily conversation in publick Coffee-houses. *Philaletbes* has more than once taken notice of him upon that account; and has likewise charged him with having from the beginning been concerned in Mr. *Robins's* papers; nor has this ever been denied.

However, let Dr. *Pemberton's* behaviour to *Philaletbes* have been what it will, although he had been heard to call him fool a hundred times, yet that can never justify *Philaletbes* in bringing a false Accusation against him. If *Philaletbes* has been guilty of this, you, Sir, I am sure, and I likewise shall be ready to join with Mr. *Robins* in condemning him.

But

\* Preface pag. xv.

But are we sure the fact is true? Is it not Mr. Robins himself, that brings a false Accusation against *Philalethes*? As you and I, Sir, know how little that Gentleman is to be trusted to in his quotations, I am persuaded you will turn to the place<sup>b</sup> he refers to; and if you do so, you will presently see upon what foundation he has raised so tragical an outcry for two pages together.

Had not people been a little accustomed to see into what a heat this writer can work himself upon the slightest grounds, there is scarce any reader but would be surprised at his talking of Dr. Pemberton's having UNFAIRLY procured this passage to be changed, of the supposition of a FRAUD, of its being little less than madness in Dr. Pemberton to have endeavoured to DECEIVE Sir Isaac Newton, of the INFAMOUS comment of *Philalethes*, of the shame with which his groundless CALUMNY ought to have covered him, of this wild and WICKED imputation, of so strange an ACCUSATION, of ACCUSING Dr. Pemberton of a crime little short of FORGERY, when even what Mr. Robins himself lays down<sup>c</sup> as a foundation for all this clamour, is not an accusation, but an insinuation only.

But what is still stranger, in the passage he refers to, there is not so much as an insinuation, that Dr. Pemberton either changed, or unfairly procured the passage to be changed. On the contrary, it is there said, that Sir Isaac Newton himself made that change; and all the insinuation is, that he made it, not of himself, but by some body's persuasion. The words of *Philalethes* are, " Sir Isaac, was by  
" some means or other prevailed upon to change  
" the word *perplexas* into *longas* in the last edition  
" of the *Principia*."

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Having

<sup>b</sup> Republic of Letters, August 1736. pag. 178.

<sup>c</sup> Preface, pag. xiv.



Having now satisfied yourself what this *wild, wicked, infamous, groundless calumny* of *Philaetbes* was, if you have a mind to see how far *he* persisted in it, you need only turn to the *Appendix* to the *Republick of Letters* for *December* 1736, pag. 13, 14, 15. And if you care to give yourself the trouble of comparing those pages, and page 37 of the *Appendix* for the *September* before, with pag. 14, 15 of this *Preface*; you will, I believe, be not a little surpris'd, that a writer so jealous of his reputation, who upon the least question of his integrity, so loudly exclaims of *gross and unprovoked abuse, of rash and groundless calumny, of charging him with wilfully misquoting and misrepresenting*, should yet so incorrigibly persist in his old method of quotation, after all the admonitions which both *Philaetbes* and I have given him.

You and I, Sir, have often taken notice of the difference between the two antagonists, *Philaetbes* and Mr. *Robins*, in the manner of their quotations. The former is so scrupulously and religiously exact in citing the very words of his author, that he appears more like a conscientious evidence in a court of justice, than a controversial writer: while the other, not regarding either the example set him, or the rules laid down on purpose for him, nor the repeated shame he is expos'd to upon detection, nor the serious admonitions so often given him, nor the stinging railleries, in prose and in verse, he so frequently meets with, can hardly ever be prevail'd upon to quote truly and fairly the words of his opponent, but instead thereof gives such a sense of them only, as he thinks most for his advantage. Nor are they the words of his opponent only, that he thus misquotes and misrepresents, he does the same with what is written on his own side, nay by himself.

For

For instance, and a very signal and flagrant one it is, the person here misquoted and misrepresented, is not *Philaletbes* only, that were less to be minded; but it is Dr. *Pemberton*, that very Dr. *Pemberton*, who has all along been the support and great encourager of Mr. *Robins*, who on his account has been so much exposed to the rudeness and folly of *Philaletbes*, has been treated by him with so many marks of contempt, and makes so dismal a figure in his verses; to all which ventilations of this author's spleen he was no otherwise intitled, than by the friendship, with which he was known to honour Mr. *Robins*, except only, that he unhappily fell into the same prevarication with Mr. *Robins*.

Of this gentleman it is said<sup>a</sup>, “ Notwithstanding the improbability of this charge, (of fraud, or forgery) Dr. *Pemberton* thought proper to take notice of it publickly, and did endeavour to obviate the suggestions of this author (*Philaletbes*) by assuring him, that the alteration in question had been inserted by the express direction of Sir *Isaac Newton*. ”

Now, Sir, I think, one may justly say, that the very supposition of such an answer, as Mr. *Robins* here imputes to his friend, was sufficiently ridiculous: for by the words of *Philaletbes* above cited it appears, that he did at first suppose, not Dr. *Pemberton*, but Sir *Isaac Newton* himself to have made this alteration. But what is worse, Dr. *Pemberton* did not take notice of any such charge, did not endeavour to obviate such suggestions, and did not assure *Philaletbes*, that the alteration in question had been inserted by the express direction of Sir *Isaac Newton*: but the whole is a meer invention of Mr. *Robins*. The words of Dr. *Pemberton* are<sup>b</sup>, “ *Philaletbes* has

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“ made

<sup>a</sup> Preface, pag. 14.  
Letters, September 1736. pag. 37.

<sup>b</sup> Appendix to Republick of



“ made a difficulty in relation to the motive, that  
 “ induced Sir *Isaac Newton* to alter,” &c.

You see, Sir, Dr. *Pemberton* himself, the person most concerned, says not a word of any such charge brought against him, nor of any such *unexpected attack on his character* : it could not therefore be in consequence of such attack, that the Doctor engaged in dispute with *Philaethes*, as Mr. *Robins* idely pretends<sup>c</sup>. The truth is, that this gentleman being grievously embarrassed by the repeated challenges of *Philaethes*, Dr. *Pemberton* stepped in to his assistance, in order to put the controversy upon a new footing, and to carry it on under a different name.

It may here be expected, I should take some notice of those two stories in the *Preface*<sup>d</sup>, which immediately affect my self. To the best of my remembrance, they are imperfectly related, and in some points, untruly.

About five years ago some passages in a paper of Mr. *Robins*, were shown to me, if I remember rightly, at *Batson's* Coffee-house ; and a question was put to me, whether I should take it ill, if those passages were printed, it being intimated, that *Philaethes*, against whom they were designed, might possibly be some friend of mine : and indeed, several persons were then guessed at, all of which happened to be my friends. To this, without taking notice, whether or no I knew who *Philaethes* was, I gave for answer, that I should not at all take it ill, and I thought no body else could justly do so. But I added, that as I had read the controversy between *Philaethes* and the Author of the *Analyst*, with some attention, it seemed to me that in one or two passages Mr. *Robins* imputed opinions to *Philaethes*, which, if I understood his meaning, that gentleman did not hold, and for this I gave my reasons.

Also,

<sup>c</sup> Preface, pag. xvi.

<sup>d</sup> Pag. iv, v, vii, viii.

Also, I took notice, that Mr. *Robins* did not rightly explain Sir *Isaac Newton's* first *Lemma*.

What I said upon the passages relating to *Philaletbes*, I suppose, appeared so plain to Mr. *Robins's* friend, if not to that gentleman himself, that soon after I was acquainted they were struck out of the copy. But when I desired to talk with Mr. *Robins* about the *Lemma*, before the papers went to the press, as imagining I could convince him that he was in the wrong, answer was made, that the question was not whether I thought him in the right or in the wrong, but only whether I should take any thing amiss; to which I replied as before.

Upon talking with another friend of Mr. *Robins* a day or two after, I repeated my desire to talk with Mr. *Robins* about his explanation of the *Lemma*, before his papers went to the press: but was told that could not be, for that the part of the papers where the *Lemma* was spoke of, was to go to the press that afternoon; from which I concluded, that Mr. *Robins* had begun to print when I was first spoke to; at least I do not remember, that any offer was made to me of *letting the whole design fall, if I desired it*. Had any such offer been made, I had at that time so much regard for Mr. *Robins*, that I think I should at least have desired him to stop the design, till he and I had examined the *Lemma* together, in order to prevent his exposing himself in the manner he has since done.

As to the second application made to me near a year after, it may easily be judged, that I, who gave these gentlemen no reason to think I had any influence over *Philaletbes*, or so much as knew who he was, could neither comply with nor reject their proposal. To say truth, I thought it a very weak one. For had *Philaletbes* been any friend of mine, and I had ever so much influence over him, yet, after Mr. *Robins* had taken the liberty of acting as  
he

he thought fit the *April* before, in declining the principal point in dispute, though invited to discuss it, in starting many new points the better to hide his prevarication, and in reviving those very points he had before struck out of his papers upon my representation; I should have thought it utterly unreasonable to preclude *Philaethes* in his answer; especially, as one part of it was already printed, and the other in all probability then in the press, having been deferred only for want of room the month before.

But how comes it to pass, that so much is said of *Philaethes* in this *Preface*? In *December* 1736, Mr. *Robins* took a solemn leave of that Gentleman in these words, "I hereby promise *Philaethes* to give him no farther trouble," and yet the controversy between them is now ripped up a-fresh. But to let alone breach of promise, where is the decency or generosity of insulting one that is *absolutely silenced*? 'Tis like trampling upon a dead man. Besides, it would become Mr. *Robins*'s modesty to remember, that he himself had been *absolutely silenced* by *Philaethes*, long before *Philaethes* was silenced by Dr. *Pemberton*. And with what regard to truth *Philaethes* has been so often said to be *absolutely silenced*, plainly appears from his declaration\*, "UNLESS Dr. *Pemberton* shall think fit to revive it (the controversy) by giving his so long demanded explication, I shall not judge it worth while to take notice of what he may hereafter write upon this subject." Now, as Dr. *Pemberton* could never be brought to give this explication, notwithstanding all the challenges and studied provocations of his adversary, the difference between the silencing of Mr. *Robins* by *Philaethes*, and the silencing of *Philaethes* by Dr. *Pemberton*, was plainly this; Mr. *Robins* was forced to be silent, because

\* Works of the Learned, July 1737. pag. 79.



because he durst not come to a distinct explanation of the *Lemma*; and *Philaethes* chose to be silent, because he found it utterly impossible to bring Dr. *Pemberton* to such an explanation.

Had Mr. *Robins* thought fit to do this, when he was first invited, I will not say challenged to it, in *January*<sup>f</sup> 1736, it would have been a better proof of his adhering to his first sentiments, than the belief of *any one person* how *impartial* soever: he would then have given no reason to think, that he continued to *defend* his *own opinions*, after he *was fully satisfied of their falshood*: and consequently, it is probable the dispute would have been carried on in another manner. For till that Gentleman in the *April* following, instead of accepting this civil though pressing invitation, began manifestly to prevaricate, you remember, Sir, *Philaethes* treated him with the utmost civility. I must needs say, the dispute was managed on both sides in a manner becoming gentlemen and scholars, nothing being said on either side that could justly give offence to the opposite party.

But, what shall we say, Sir? So it happened, when, at that fatal *Æra*, Mr. *Robins* began to prevaricate, *Philaethes* began to lay down rules, and to write verses; and the more Mr. *Robins* prevaricated, the more verses were written.

The goodness of the rules is not disputed; and if Mr. *Robins* has not broken those rules, what need he be so angry at them? He seems to complain of *personal* reflections: but that, I think, can only arise from his not knowing the difference between entering into the personal character of an adversary, and the exposing his behaviour as a writer.

As to the goodness of the verses, or the propriety or *absurdity of their application, or the stiff and*  
*cumbrous*

<sup>f</sup> Republick of Letters, pag. 78. lin. 19, &c. pag. 80. lin. 24, &c. pag. 84. lin. 8.

*sumbrous band, that perpetually appears in those motly extravagancies and grotesque decorations, I should think, neither Mr. Robins, who unhappily is too often the subject of that wretched mummery, nor I, who have discovered such an absolute want of all taste, as to think that Homer, and Virgil, and Milton may stand comparison with any later poet, can be a proper judge. I shall therefore only say, those verses seem to me to be written by a person far removed from that perturbation of mind and vehemence, which is usually styled anger. A friend of yours and mine, whose judgment and good taste is known to all the world, professes, that to him Philaethes appears so far from an angry person, that the best image he can form of him, is that of a jolly, fat, good-humoured fellow,*

*That laughs and shakes in Rabelais elbow chair.*

Indeed, you and I, Sir, who have laughed so much upon reading those verses, have often wondered how Mr. Robins could possibly take it into his head, that Philaethes was in a passion when he wrote them. Angry persons are more serious. But in the *Preface*<sup>2</sup> before me I meet with a very deep and curious observation, that solves the difficulty. "It is," says this profound Philosopher, "constantly the wish of all, who are under the influence of that weak passion, (anger) that the object of their displeasure might suffer the same uneasiness, as they feel within themselves." Now from the high resentment, which Mr. Robins still expresses at the virulent defamations, *pus atq; venenum*, of those abusive verses, no doubt can be made but when he first read them, or when they were first construed to him, he was so far from being very far removed from that perturbation of mind and vehemence, which is usually styled anger, that he was strongly under the influence of that weak passion, or in plain



plain *English*, he was very angry. Therefore, by his own observation, he could not but wish, that *Philaethes* might suffer the same uneasiness, as he felt within himself: and as it is natural to believe what we wish, he was easily led to think, that *Philaethes* actually suffered that uneasiness. But had the dispute been carried on *viva voce*, I dare say my cobbler would have been of another opinion.

Here, I should be sorry Mr. *Robins* were offended at my supposing, that possibly the verses might have been construed to him. Far be it from me to surmise, that a gentleman, whose *English* shews so much skill in grammar, should not be able to construe *Latin*. But there being two or three very gross mistakes in the little he has produced from those verses, 'tis in tenderness to him, that I am willing to think them the blunders of some body else. There are some persons so fond of their talent for explication, that they will needs explain to you what you understand much better without them: and the mistakes I speak of, can, I think, only have arisen from some one of these wrong heads, so intirely taken up with modern poetry, that no room is left for any taste or comprehension of *Virgil* or *Horace*.

It may possibly be thought, that Mr. *Robins*, with all the modesty I allow him, can hardly pay so great a deference to another person, as implicitly to submit to his judgment, and that in a point too, where the consequence is his being taken for an old woman. The objection, I must grant, is of force; and it will not be easy to solve it, unless we suppose this gentleman to be a little in the case of *Mark Antony* in *Plutarch*, whose guardian *Genius*, though infinitely bold and daring in all other company, was yet most shamefully cowed and overawed, when in presence of that one stronger *Dæmon*, which attended the chief of the *Triumvirate*.

The

The boldness and sublimity of this author's genius may appear from innumerable passages of his writings, and even those two or three, which I have had occasion to produce, may suffice to shew, that notwithstanding the care and circumspection he uses in penning and reviewing his labours before publication, and the corrections he afterwards applies by the assistance of his two learned friends, it is no way possible for him to lower and let himself down to vulgar comprehensions. The noble images and descriptions he every where presents us with, are either wholly his own, or if borrowed from the antients, consist not of their very words *pedantically applied*, as in the writings of *Philaethes*; but are so much embellished by the strength and liveliness of his imagination, that the imitation, beyond all comparison, out-does the original. For instance, how low and cold is the image of the superannuated stallion in *Virgil*!

*Frigidus in venerem senior, frustra; laborem  
Ingratum trahit, Et si quando ad pralia ventum est,  
Ut quondam in stipula magnus sine viribus ignis,  
Incassum furit.*

On the other hand, *What can be wanting to complete the sublime of this noble imitation?*

“ These frozen conceits, and labour'd absurdities - - - - - are evidently the strained efforts of a lethargic imagination awkwardly toiling under the teasing impulses of a most demure and solemn vanity.”

You observe, Sir, that not only the thought, but the very wording of the period,

*That, like a wounded snake, drags its slow length along,*

does equally contribute to heighten the description.

Now what pity is it, that one, who is master of such a cataract of elocution, such a hurricane of sublime,

sublime, and especially so compleat a *Thesaurus* of the vituperatory *style and diction*, which seems to have exhausted the *phrasology* of that celebrated *Emporium* below the bridge, should squander it all away upon so inconsiderable a person as my self! One half of this, well managed, might have blown up a prime Minister.

I must confess indeed, this honour is not all my own: my friend Dr. *Smith* has a large share of it. And here, you, Sir, who are well acquainted not only with the reputation, but with the amiable private character of that gentleman, cannot but wonder how it could enter into the heart of any man breathing, but especially of one, who looks upon the charge of *conducting his animadversions with passion and abuse*, as so heavy an imputation, to treat him in so outrageous a manner. At first, it seems, he was suspected of being the associate of *Philaletbes*. Be it so. But this candid writer, who had publickly declared <sup>i</sup> *he was thus free, that he might do him the justice to give him an opportunity of acquitting himself, if he were falsely accused*, does now actually acquit him. What amends therefore does he make him? The Roman Bravo would have begged his pardon: but this good-natured *Englishman* treats him ten times worse than ever. If this be Mr. *Robins's justice*, the best way for me is to continue under suspicion.

This proceeding brings to my mind a story I have somewhere read, of that just and gracious Emperor, *Tiberius*. He was one day sitting upon the examination of some state prisoners, when word was brought him, that such a person attended without. This happened to be one of his old *Rhodian* acquaintance, who had a great share of his friendship, when he led a private life upon that island, and was just come over to *Rome*, upon a  
kind



kind invitation he had lately sent him. But now, being extremely intent upon the business before him, he took it for granted this was one of the *suspected* persons he had caused to be apprehended, and thereupon gave orders he should be instantly put to the torture, which, no body daring to undeceive him, was accordingly done. Soon after, finding what a terrible mistake he had committed, and not being able to bear the sight of his friend in that torn and mangled condition, the good Prince was so overwhelmed with shame and sorrow, that - - - - he ordered him to be privately murdered.

It is now high time to have done with this gentleman's *Preface*, and to come to his *Full Confutation*; in this you will find the accuracy of his reasoning not at all inferior to the accomplishments of his style in the former.

And here, Sir, I flatter my self, you will by no means agree with this learned person<sup>k</sup>, that in explaining an intricate physical subject, where different causes concur in producing the phænomena, it is any fault to consider first the principal cause alone, and to see what effects will follow from that singly taken. When this is rightly comprehended, you will then think it time enough to proceed to any other cause of less moment, and to observe what change in the effect is thereby produced. This you know, Sir, is agreeable to the practice of the best and clearest writers, who suppose the planets to move in Ellipses round the center of the Sun considered as at rest in one focus; and that heavy bodies in falling through the air are uniformly accelerated; neither of which principles are strictly true, but are attended with some limitations. If these limitations were inserted into the general doctrine, and treated of jointly with it from the beginning, after the manner of some injudicious  
and

<sup>k</sup> Full Confutation, pag. 3.

and obscure writers, the whole would be rendered much less intelligible.

The principle I at first assumed in my *Essay*, that the light is uniformly spread through the circle of dissipation, though it be not strictly true, as I gave repeated notice<sup>1</sup>, yet is near the truth, and consequently, the determinations drawn from this principle are also near the truth, except in some particular cases where I gave that notice, and which are afterwards deduced from a particular cause.

My not having considered the sights made use of by the antients, in the place where I ought to have done it, I have already owned to be an omission, and if this gentleman will needs have it to be an oversight, or a blunder, I shall not contend with him.

By the spreading of the light through the circle of dissipation, a black round spot ought to appear in the center of a small circle drawn upon white paper, when the paper is held very near the eye, as I have shown in the *Essay*<sup>m</sup>: and that such a spot does actually appear, any one may experience. This appearance, I think, Mr. *Robins* has never seen; and therefore I shall here give him some directions about it more particular than before.

To my eye, which does not see an object with perfect distinctness at less than 40 inches distance, the O in the word *Vision*, in his title page, is too large to exhibit a black spot in the center, as is likewise the O in the word *Confutation*, when held so near the eye that my nose touches the paper, except by candle-light, or a weak day-light: but the O, in the following word *of*, exhibits the dark spot, at that distance, in a good light. To a person shorter sighted, either the circle must be smaller, or the paper held nearer, or the light must be weaker. But by some of these means the spot will appear to

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any

<sup>1</sup> *Essay*, art. 66, 78, 197, 220.

<sup>m</sup> Art. 92.



any eye, with a penumbra round it, as described in fig. 48. of my *Essay*: and it ought to appear so, when the circle of diffipation is equal to the image of the circle we look at, without any *corrugations* in the eye.

To this Mr. *Robins* objects<sup>n</sup>, that “ the spaces “ G C H E, and I F K D, will be deprived of “ light.” It is true: but if the retina be drawn between G I and the crystalline, no part of the retina will fall within those spaces, and consequently no part of the retina will be wholly deprived of light.

He objects farther<sup>o</sup>, that “ all without those “ spaces will be so much illuminated, that the axis “ of the eye can no where be deprived of light “ sufficiently to produce so distinguishable a degree “ of darkness,” as our spot requires.

In order to examine this objection, let us suppose the radius of the small white circle we look at, to be one, or unity; and the breadth of the black annulus round it, to be also one: and let the radius of diffipation be equal to the radius of the image of the whole circle composed of the white circle and black annulus, painted confusedly upon the retina, as in art. 92. of the *Essay*.

Then will a physical point in the center of the circle scatter its light over the whole image, that is, over the space four: and on the other hand, the image of this central point upon the retina, will receive part of the scattered light from every physical point within the white circle, that is, from the space one; but will receive none at all from the dark annulus, that is, from the space three. Therefore the image of this central point will have but a quarter part of the light, which it would receive by distinct vision, or even by indistinct vision if the annulus were as white as the rest of the paper, it being

<sup>n</sup> Remarks, pag. 94.

<sup>o</sup> Ibidem.

being deprived of three quarters of its light bestowed upon the dark annulus ; from which it receives none in return.

Now any other physical point, at some distance from the center, will also scatter its light as wide as the light of the central point is scattered ; but in return will receive a greater quantity of light from the other points within and without the annulus, being robbed of a less and less part of its light by a less and less part of the black annulus, as the point is more and more remote from the center. Consequently, the black spot must appear surrounded with a penumbra growing gradually weaker from the center.

Perhaps, this may be more easily understood, if we represent it in another manner.

Instead of a black annulus drawn upon white paper, let us imagine a white annulus drawn upon black paper : and let the paper be held so near the eye, that the circle of dissipation may be equal to the image of the black circle, or rather a very little exceed it. Then will every point of the inner edge of the white annulus throw a part of its light upon the center of the image : and if the paper be held still a little nearer, so that the circle of dissipation may be equal to the image of the black circle and white annulus taken together ; then every point of the white annulus will bestow a part of its light upon the central point. A white spot must therefore appear in the center. And as every other point of the image can receive the scattered light from a part only of the white annulus ; and as this part will be less and less by how much the point is more and more distant from the center ; the white central spot will be surrounded with a fainter white gradually decreasing after the manner of a penumbra.

When Mr. *Robins* has seen and considered these two contrary appearances, it is to be hoped he will

acquiesce in the solution I have given : and to set him an example of ingenuity, I here acknowledge a mistake I committed in my *Reply*.

I there affirmed, that the spaces G C H E, and I F K D would not be deprived of light ; which I now find, is not true. The occasion of my mistake was, that I understood Mr. *Robins* to mean, that the images C E, F D were represented upon the retina. And had they been so, they would not have been deprived of light in the case I had proposed, in which the radius of dissipation was equal to the radius of the image. I must indeed confess, that he did at first call them *distinct images* ; and consequently, they could not fall upon the retina, but must lie a considerable distance behind it, as I now find he intended. But as, in this case, no argument could be drawn either from those images, or from the spaces G C H E, I F K D which depend upon those images, against my representation of the confused image upon the retina, which lies more forward than the beginning of those spaces ; I thence too hastily concluded, that by *distinct images* Mr. *Robins* meant those images upon the retina, that were occupied by the centers of the several pencils, such as in treating of this matter in the *Essay* I had called <sup>p</sup> *true images*, which would be *distinct*, did we look at the object through a small pin hole. However, I have now given the right answer.

When this gentleman has reconsidered the case I proposed, he will easily see, that what he represents <sup>q</sup> as the same with mine, is quite a different one, and depends upon different considerations, into which it is not necessary here to enter.

But that the appearances I have described, of objects multiplied by indistinct vision, are not occasioned



sioned by any <sup>r</sup> *corrugations in the eye*, he may satisfy himself by an experiment very easily made, and withal so convincing, that it deserves the name of *experimentum crucis*.

Let him stand at any distance from a window, between the limits of distinct vision to his own eye; and let a pin be stuck up perpendicularly in that window. Then let him hold a fine needle parallel to the pin, and at such a small distance from his eye, that it may exhibit the appearance of two or more needles.

Now, let him fix his eye upon the needle, and attend at the same time to the appearance of the pin: or let him fix his eye upon the pin, and at the same time attend to the appearance of the needle: and he will in both cases see the needle double or treble, and the pin single. — But a *corrugation in the eye*, or any <sup>s</sup> *inequalities analogous to those, that appear in a common multiplying-glass*, would make the pin appear double or treble, as well as the needle.

Farther, if the distance of the pin from his eye be that, at which he sees an object distinctly with the most ease, or the distance at which he would choose to read a middling print; then in looking at the pin, he must acknowledge the eye not to be at all strained: and yet the needle will then appear double or treble. This he has declared <sup>t</sup> *to be plainly impossible*: but before we reject matters of fact and observation, upon the credit of any theory, we ought to be very sure that our theory is true. In the true method of philosophising nothing ought to be laid down as theory, but what is deduced by just reasoning from observation or experiment: and where any doubt arises about the justness of the reasoning, the theory must be brought to the test of experiment. If these two differ; 'tis not the ex-

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periment,

<sup>r</sup> Remarks, pag. 94. Full Confutation, pag. 7, 51.

<sup>s</sup> Full Confutation, pag. 6.

<sup>t</sup> Ibid. pag. 7.

periment, but the theory, which ought to be rejected.

Had Mr. *Robins* taken this method, he might long since have determined, by the help of the 97th article of my *Essay*, whether the dark central spot was owing to *corrugations in the eye*, or to the dissipation of the rays. That article runs thus:

“ When the central spot begins to appear, if  
 “ the eye continue attentively fixed upon it, or if  
 “ the sun break out on a sudden from a cloudy sky,  
 “ or if at night a candle be snuffed so as to burn  
 “ more brightly; in any of these cases the spot  
 “ will disappear, and a small white circle will ap-  
 “ pear in the room of it. For in all these cases,  
 “ the pupil contracts to a narrower aperture, and  
 “ the radius of dissipation, which is always pro-  
 “ portional to that aperture, is thereby lessened,”  
*&c.*

I am desired<sup>v</sup> to reconsider the case of the dark angle, drawn in fig. 36, 37. of my *Essay*, and am told, the space between the two appearances, which I represent as fully illuminated, ought to have been obscured, for the penumbra must encompass the object. But, upon reconsidering this case, I find, as I found at first, that the penumbra in that place must be so weak, as not at all or hardly to be visible: and Mr. *Robins* may find the same, if he is pleased to attend to article 76 of the *Essay*.

If this gentleman is not yet satisfied with what I have said about my defence of *Hevelius*, I hope however, he will allow, that a mistake is more pardonable in him that defends an author unjustly attacked, than in one that unjustly attacks him.

Mr. *Robins* in his *Remarks*<sup>w</sup> had been pleased to speak of me in these words, “ After informing us,  
 “ that he had deceived himself by certain trials to  
 “ favour the singular opinion of Mons. *De la Hire*,  
 “ ——— he

<sup>v</sup> Ibid.

<sup>w</sup> Pag. 6.



“ — he tells us, that Dr. *Porterfield* by experiments better contrived has caused him to change his mind, &c.”

Now, Sir, supposing this had been true, what is the fault I am here reproached with? Is it, that I was once in a mistake? Alas, I am not ashamed to say, I have been in many. Or is it, that I changed my opinion, when I had good reason given me to the contrary? I hope I shall always do so. Or am I blamed for owning my conviction? I take this to be a wiser course, than that of defending what I know to be false.

But the misfortune is, that not a word of this charge is true. In my *Reply*<sup>y</sup> I gave Mr. *Robins* to understand, that I had no where *informed* him I *had* so *deceived* myself; that I never *told* him, Dr. *Porterfield* *had* caused me to change my mind. What answer does he make to this? Does he offer to shew, where I have *informed* him of the one, or *told* him of the other? No, he does not pretend to it. Has he therefore ingenuity enough to make any excuse, to acknowledge that he has misrepresented me? Not a word of this. And yet, Sir, this is the man, who in his *Preface* speaks with so much horror of one, *that thus wickedly violates the impulses of his own Conscience*; the man so tender of his reputation, that he is ready to fall into agonies upon the least insinuation of his *misquoting* or *misrepresenting* his antagonist.

But though Mr. *Robins* can no way make good his charge, you must not expect he should wholly drop it: it is not his way to give up any thing he has once maintained. I had farther told him in my *Reply*<sup>z</sup>, that the *account* I gave of my trials, *was directly contrary to Mons. De la Hire*. And to this he answers<sup>a</sup>, “ it is not so: it is indeed quite contrary

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“ to

<sup>y</sup> Pag. 8.

<sup>z</sup> Ibidem.

<sup>a</sup> Full Conf. pag. 10.

“ to the common opinion, but very consistent with  
 “ M. *De la Hire*’s.”

Here, Sir, I must bespeak your attention to the argument, by which this clear headed reasoner proves my account to be *quite contrary to the common opinion*; and after that we shall examine into its consistency with the opinion of *Monf. De la Hire*. He goes on thus,

“ What<sup>b</sup> he says, that the nearest distance for  
 “ distinct vision he found to be forty inches in his  
 “ eye is irreconcilable with the common do-  
 “ ctrine.”

If this be true, so much the worse for the *common doctrine*. I never heard that this common doctrine was made an article of faith: and had it been so, I should have been apt to believe my own eyes. When I see an object double, it would be hard to persuade me that I see it single. But how will this gentleman prove my observation to be *irreconcilable with the common doctrine*?

“ That,”<sup>c</sup> says he, “ always supposes objects  
 “ to be seen perfectly distinct at that distance,  
 “ where a person will chuse to hold them to view  
 “ them with greatest advantage.”

Here it is tacitly taken for granted, that *to view* an object *with greatest advantage* is the same as *to see* it *perfectly distinct*. But this is not always true. A small object may be seen with greatest advantage, when held too near the eye for vision perfectly distinct; the disadvantage arising from a little degree of indistinctness being more than compensated by the advantage of a greater apparent magnitude.

However, let this pass for the present, and in order to examine this goodly argument to the bottom, let us agree with Mr. *Robins* in supposing *objects to be seen perfectly distinct at that distance; where a person will chuse to hold them to view them with greatest*

<sup>b</sup> Ibid.

<sup>c</sup> Ibid.

*greatest advantage.* What use will he make of this?

<sup>d</sup> “ Now,” says he, “ no one with a sound eye “ holds an object at more than a yards distance for “ that purpose.” No truly, nor with an unsound eye neither. But this is matter of necessity, not of choice. The human arm is but a yard long.

I have shown\*, that vision perfectly distinct has its limits ; that the nearest limit to a young person is five or six inches : but that this distance increases with age. Mr. *Robins* does not deny this ; and if he did, adieu to spectacles. What should hinder then, but this distance may in time come to 40 inches, as I found it in my eye, by a great number of trials, about five years ago ? Then, a very little alteration farther in the eye will bring it to five or six feet. Let now an old man, whose nearest limit for perfect vision is six feet, look at a picture without his spectacles ; and it may be he will hold it at arms length. He *chuses to hold* it as far off as he can, because it would be more indistinct if he held it nearer ; but if his arm were longer, he would *chuse to hold* it farther off : and if it be hung against a wall, he will *chuse* to stand two yards off, in order *to view* it *with the greatest advantage*, unless the parts of the picture be too small.

But whatever be the distance, at which such a person sees an object perfectly distinct, there is nothing in this *contrary to the common opinion* : he may still alter the conformation of his eye so as to see distinctly at greater distances.

Let us next examine, whether my seeing an object distinct sometimes at 40, sometimes at 50, 60, 90, or more inches distance, be, as this gentleman affirms, *very consistent with Mr. De la Hire's opinion.*

That

\* Ibid.

\* Essay, art. 142, 143.



That learned person <sup>f</sup> asserts, that in viewing an object through two pin-holes placed near together, at some one distance the object appears single, and at greater or lesser distances always appears double. From this he infers, that to a given eye the distance for distinct vision is one determinate, invariable distance: and consequently, that the eye cannot alter its conformation in viewing objects at different distances.

Now by the <sup>g</sup> account of my tryals it appears, that I saw the object distinctly sometimes at 40 inches distance, and sometimes at 90: consequently, my eye must have altered its conformation in those tryals. Therefore my experiments and the conclusion drawn from them, are so far from being *very consistent with*, that they are directly contrary to the experiment and conclusion of Monsieur *De la Hire*.

You will hardly think it worth while, to take notice of the little artifice of foisting in the words, *at will, at pleasure, accidental, at the choice of the eye*, none of which had ever been mentioned either by me, or by Monsieur *De la Hire*. For if the eye cannot at all change its conformation, it can neither change it *at pleasure*, nor *accidentally*. Nor is it at all to the purpose to talk of Monsieur *De la Hire's supposing, that age will create such a difference*. The difference in my tryals was not created by age, but would happen in the compass of a few days, nay of a few hours: and the greater distance would sometimes precede the lesser, as well as at other times the lesser would precede the greater; the reason of which variations, when one eye alone is used, is well explained in Dr. *Porterfield's Essay*.

Mr. *Robins* farther adds<sup>h</sup>, “ that the minute discrimination insisted upon by our author, between  
“ two

<sup>f</sup> Des differens accidens de la *vûe*, Partie ii. pag. 295.

<sup>g</sup> Essay, art. 105.

<sup>h</sup> Full Conf. pag. 11.



“ two sorts of distinct vision, the perfect and the  
 “ less perfect, is of little or no use in any, but M.  
 “ *De la Hire*’s opinion,” &c.

But here I must remind him, that perfect vision has its limits. And in all cases out of those limits, this *discrimination* is of just the same use in the common opinion, as it is in the opinion of Monsieur *De la Hire*.

I believe, Sir, you are by this time heartily tired with the opinion of Monsieur *De la Hire*, and for my own part I could almost sit down with *Philalethes*, to deplore my own misfortune in having to do with a writer, who, in a single sentence, almost with one dash of his pen, makes no conscience of cutting me out work for several pages. Were it not better to give up one untruth, at first perhaps inadvertently asserted, than thus to add error to error in defending it?

Were it not better also to give up that other untruth, of saying I had told him, that Dr. Porterfield had caused me to change my mind, than to persist in it after so weak a manner? Could he prove, that this learned gentleman had really caused me to change my mind, yet it were untrue to say I ever told him so. But how does he prove it? He pretends that <sup>i</sup> the phrase, in which I mention Dr. Porterfield’s experiments, is exactly the same, as it must have been, had I my self been before of a different opinion, &c. But, I believe, Sir, you will easily allow, that my words, <sup>\*</sup> I fully acquiesce in the proof he has given, are very consistent with my having been of the same opinion before, though, for the proof of it, I had not so good an opinion of my own experiments, as of his, for the reasons given in my *Essay*.

As to my manner of accounting for the change of conformation in the eye, in order to see distinctly  
 at

<sup>i</sup> Ibid. pag. 10.

<sup>\*</sup> *Essay*, art. 105.

at different distances, I am not at all sollicitous whether Mr. *Robins* or his instructor approve of it or not. Those, who have themselves been unsuccessful in any attempt, are not the most impartial judges of the performance of another. I have laid down some postulata, and those being granted, all the phænomena may be accounted for, which, I think, is more than has been ever done before. But whether those postulata are reasonable, or not, must be left to the determination of the learned.

In my *Reply*, after reciting some very weak, though very positive <sup>1</sup> assertions of Mr. *Robins*, concerning the proportion, which he pretended Sir *Isaac Newton* had assigned between the extent of the fit of easy reflexion and the interval of the fits, I had taken the liberty to say, that <sup>m</sup> *not a tittle of this was true*; and added, that Sir *Isaac* was so far from assigning the proportion pretended, or any proportion whatsoever between the extent of each fit of easy reflexion and the interval of the fits, that he has never once considered, or so much as mentioned the extent of the fit of reflexion, or of the fit of transmission. And I did then call upon Mr. *Robins* to produce the passage of Sir *Isaac Newton's Opticks*, wherein such proportion between the extent of each of these fits is assigned, or where that extent is so much as mentioned.

<sup>n</sup> “ Now in answer to this confident challenge,” we are told, “ it is sufficient to exhibit the following quotation.”

That is, Mr. *Robins* being challenged to make good what he had again and again affirmed, by producing a passage where such proportion between the extent of the fits is assigned, or where that extent is at all mentioned, thinks it *sufficient to exhibit* a passage where no proportion at all is assigned, and where that extent is never mentioned.

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<sup>1</sup> Pag. 13, 14.  
tion, pag. 22.

<sup>m</sup> Pag. 14.

<sup>n</sup> Full Confuta-

The passage produced is the 12th proposition of the 2d book of *Opticks*, and the latter part (the first not being so convenient) of the explanation of that proposition; from which explanation, by an argument of a page and half, Mr. *Robins* comes at last to infer°, not that Sir *Isaac Newton* has assigned, or mentioned, but that *he has indubitably supposed that equality between the extent of the fit of transmission and reflexion, which Dr. Furin has so rashly denied.*

So that at last, you see, Sir, this proportion of equality, which *was the result of mature deliberation in Sir Isaac Newton*, and is *necessary towards producing the appearances*, so necessary, that my alteration of it *absolutely destroys the whole theory established by Sir Isaac Newton, setting it at variance with almost every appearance it is intended to solve*, this so important a proportion, which a writer of common circumspection would not only have expressly assigned, but would again and again have inculcated and repeated, is no where, not so much as once, after the most diligent search, by three observers deservedly famous for their perspicacity and *careful attention*, to be found in the book of *Opticks*, till by the greatest good fortune in the world it was discovered dormant in this explanation. And yet how severely have I been reprimanded for having *so rashly denied it!*

But after all, to give this gentleman his own way, how will he make it out, that the proportion of equality between the extent of the fits is, I will not say *indubitably supposed*, but at all supposed in this proposition, or its explanation?

He tells us<sup>p</sup>, “ This proposition is laid down  
 “ universally of every ray of light without any  
 “ exception; and the method directed to find the  
 “ subsequent fits of reflection and transmission is to  
 “ multiply

° Ibid. pag. 24.

p Full Confutation, pag. 23.



“ multiply continually the interval between the  
 “ first fit of reflection and the surface, without  
 “ the least intimation, that this method is to be va-  
 “ ried in any ray whatever.”

Here I readily agree with Mr. *Robins* that the proposition is universal, forasmuch as, in every ray of light whatsoever, the fits of transmission and reflexion will return alternately at equal distances. Nor shall I deny, that the manner of determining the returns of the fits, which is used in this explanation, will serve for all rays whatsoever.

But what is *the method* here said to be directed? And by whom is it directed? By Sir *Isaac Newton*, or by Mr. *Robins*? This gentleman would have his reader believe, that this method is directed by Sir *Isaac Newton*; nay, in the following page he has the *hardiness* to call it, *the rule here laid down by Sir Isaac Newton*; and yet it is most certain, that Sir *Isaac* gives no such direction, and lays down no such rule: but the whole is a piece of the usual dexterity of this writer, who first interprets the words of his author as he thinks most for his own advantage, and then palms that interpretation upon us, as if it were a direction actually laid down by Sir *Isaac Newton*.

In the first part of the explanation, which is omitted by Mr. *Robins*, it is said, “ IF the first re-  
 “ flexion ——— be made at the thickness 1, the  
 “ rays shall be transmitted at the thicknesses 0, 2,  
 “ 4, 6, 8, 10, 12, &c. ——— and be reflected at  
 “ the thickness 1, 3, 5, 7, 9, 11, &c.”

Now here I must take notice, that Sir *Isaac Newton* in determining the fit of transmission or reflexion, always means the middle of the fit, so that throughout this explanation the ray is supposed to be in the middle of the fit of transmission, at the thickness 0, 2, 4, 6, 8, &c. and to be in the middle of the fit of reflexion, at the thickness 1, 3, 5, 7, 9, &c. so that the thickness 1 is the interval between

tween the middle of the fit of transmission, and the middle of the subsequent fit of reflexion.

This being premised, we come next to examine what is *the method* here directed, not by Sir Isaac Newton, but by Mr. Robins, to find the subsequent fits. This able geometer tells us, it is to multiply continually the interval between the first fit of reflexion and the surface. This greatly puzzles me. For that interval is 1, and by multiplying 1 continually, though for ten thousand times together, I can still make but 1; so that this method will never help me to find the subsequent fits, but I must stop at the first fit of reflexion, without any possibility of getting farther.

But perhaps his meaning is, that we are to multiply this interval by the terms of the natural series, 1, 2, 3, 4, 5, 6, &c. and that not continually, but successively and separately. If so, it would be a simpler direction, to bid us add that interval continually to itself.

Now here a fresh difficulty arises, in order to state which, and afterwards to resolve it, it will be necessary to lay down the case before us somewhat more distinctly.

The design of the *method* pretended by Mr. Robins, to be directed by Sir Isaac Newton, is not to find the first fit of reflexion, but to find the subsequent fits. The first fit of reflexion, or the interval between that fit and the surface, is supposed to be already known; and the question is, what are we to add to that interval, in order to find the subsequent fits. Is it that interval itself between the first fit of reflexion and the surface, as Mr. Robins would have it? Or is it not rather the interval between the first fit of transmission and the first fit of reflexion?

Now the explanation before us can never decide this question. All we can learn from that is, that, in order to find the subsequent fits, to the interval  
 2 between

between the first fit of reflexion and the surface, or to the distance 1, we are to add continually the distance 1: and that distance 1 is not only the interval between the first fit of reflexion and the surface, but is also the interval between the first fit of transmission and the first fit of reflexion. What therefore are we to do, when the first of these two intervals shall happen to be different from the second?

Here, Sir, is it not much to be lamented, that neither this sagacious gentleman himself, nor either of his clear headed assistants and fellow labourers, should have been aware of this difficulty? Had they been so, they could never have thought it *sufficient to exhibit this quotation only, in answer to my confident challenge*, but would have looked over the book of *Opticks* a second time, in order to find out some other passage to be joined with this; without which it will be vain to pretend, that their proportion of equality has been *indubitably supposed* by Sir *Isaac Newton*.

But to save these learned persons the trouble of hunting for what they will never meet with, I shall here give a clear and distinct answer to my own question. To the interval between the first fit of reflexion of any ray and the surface, in order to find the subsequent fits, we are to add continually the interval between two contrary fits in that or in any other ray of the same species.

For instance, if two rays of the same species, suppose of the extreme violet, pass through the surface A H in Sir *Isaac Newton's* 6th figure of the second book; and the first of these be in the middle of its fit of transmission at the point A, and in the middle of its fit of reflexion at the thickness A 2; but the second of these rays be past the middle of its fit of transmission at the point A, and consequently be in the middle of its fit of reflexion at a thickness less than A 2, suppose at  $\frac{1}{2}$  of that thickness, as in  
the



the instance laid down by Mr. *Robins*: Then, in order to find the subsequent fits in the first of these rays, to the distance A 2, we are to add an equal distance, to find the second fit of transmission; and by adding such another distance we shall find the second fit of reflexion, and so on. About this there is no dispute. But in the second of these rays, whose first fit of reflexion happens at  $\frac{2}{3}$  of the distance A 2, in order to find the subsequent fits, we must not, as in *the method directed* by Mr. *Robins*, double that distance  $\frac{2}{3}$  of A 2, in order to find the next fit of transmission, nor treble that same distance in order to find the next fit of reflexion. For then the interval between two succeeding fits in this second ray, would be different from the interval between two succeeding fits in the first ray: whereas these two rays, being perfectly homogeneous, and passing through the same medium, ought by the constant tenour of Sir *Isaac Newton's* doctrine to have equal intervals between their fits. We are therefore to add to the distance  $\frac{2}{3}$  of A 2, the whole distance A 2, in order to find the next fit of transmission, and we must again add the same distance A 2, in order to find the next fit of reflexion, and so on. By this means the two rays will have equal intervals between their fits, as they ought to have: and in the second ray <sup>a</sup> *the regularity of the following fits*, which Mr. *Robins* says, *will be totally destroyed*, will on the contrary be perfectly preserved.

To say truth, I cannot see how *the regularity of the following fits* should be destroyed, were we to follow Mr. *Robins's* own method of multiplying the distance  $\frac{2}{3}$  of A 2. For even in this case the following fits would be perfectly regular, always returning at equal distances, though less than Sir *Isaac Newton* makes them.

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<sup>a</sup> Full Conf. pag. 23.

He tells<sup>r</sup> us indeed, that “ according to the rule  
 “ here laid down by Sir *Isaac Newton*” (or by Mr.  
*Robins* for him) “ the next fit of reflexion would in  
 “ such a ray be at twice the distance of the line  
 “ 2 K from A H, that is, it would fall in that  
 “ place, where we know all the rays are disposed  
 “ to be most freely transmitted.”

But how does this gentleman *know all the rays are disposed to be most freely transmitted at twice the distance of the line 2 K from A H*? He can only say he knows it, because all the rays are disposed to be most freely reflected at the single distance of the line 2 K from A H. But is not this begging the question? My assertion, against which he is here arguing, is, that part of the rays are disposed to be most freely reflected at a much less distance: and he has just now told us, “ in Dr. Jurin’s hypothesis this is supposed to happen to great numbers of rays.”

But though neither *the method directed to find the subsequent fits*, nor *the regularity of the succeeding fits*, can be of any service to Mr. *Robins*; yet he may still insist, that as Sir *Isaac Newton* in this explanation makes no mention of any rays, but such as have their first fit of reflexion at the distance 1, that Great Man must have supposed, that all rays of the same species have their first fit at the same distance from the surface.

If he does so, we must answer, that the design of that proposition, was not to determine where the first fit of reflexion in every ray should fall out, nor whether all rays of the same species should have that fit at the same distance from the surface: but it was only to shew, that in every ray of light the fits of transmission and reflexion return alternately at equal intervals. In order to explain which, he proposes the case of a ray, which is in the middle of the fit of transmission at 0, where it enters the  
 surface,

<sup>r</sup> Ibid. pag. 24.

surface, and is in the middle of the first fit of reflexion at the distance 1, so that this distance 1 is the interval between two contrary fits: and by adding that interval continually to itself he determines the places of the succeeding fits. He might have given any other instance, where a ray was not in the middle of the fit of transmission at 0, but was either past it, or not arrived at it: but he chose to give this, I suppose, for two reasons; one, that this was the easiest and clearest case for the reader's conception; and the other, that it was the case of the greatest number of the rays, forasmuch as the rays of every species are more copiously reflected at the thickness represented by the line 2 K, than at any other.

It is therefore without all shadow of proof, that Mr. Robins infers from this proposition, that *every ray, which passes the surface, is in a fit of reflexion at the line 2 K.* And yet from this groundless inference alone, by an argument equally weak, and not drawn from the words of Sir Isaac Newton, but from a general figure only, but which, after what has been said, it is not necessary to examine, is derived that sanguine conclusion, "Sir Isaac Newton has indubitably supposed that equality between the extent of the fit of transmission and reflection, which Dr. Jurin has so rashly denied."

"Hence" (says he) "appears the justness of that supposition of ours, which Dr. Jurin has so abusively censured, I mean the taking the distances between the lines 1 I, 3 L, &c. to denote the extent of a fit of reflection in a single ray."

Certainly this must be false printed. I am very sure, I never censured this gentleman for taking more than one distance to denote the extent of a fit of reflection in a single ray. It never entered into my thoughts, that so accurate a writer as Mr. Robins,

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espe-

f Ibid.

f Ibid.

v Ibid.



especially when so well assisted, could possibly *take the distances between those lines*, distances infinite in number, for the extent of the fit of reflexion in a single ray. I did indeed <sup>w</sup> reprove him for supposing the distance 13 in the 6th figure, to be the extent of the fit of reflexion in a single ray; and the weakness of that supposition now appears plainer than ever. It was laid down in the *Remarks* not only without any foundation, but contrary to the express declaration of Sir *Isaac Newton*, who calls that distance *the extent of the colour*, or *the latitude of the colour*; and it is now built upon an inference, which we have shown is utterly precarious.

It is not therefore material to enquire, whether Mr. *Robins*, when he wrote his *Remarks*, had, as he now pretends, <sup>x</sup> *this principle* or inference, in his view. If he had, he certainly took no notice of it to his readers. He says indeed, “I shewed upon “ this principle, that Dr. Jurin’s rule for confining and varying the extent of these fits was absolutely inconsistent with the phænomena in the “ colours of thin transparent plates.” But whoever turns to his *Remarks*, will find that he never shewed it upon *this principle*, or any other, but only asserted it. It was a bare *gratis dictum*, without the least support or foundation. Nor indeed do the phænomena in the colours of thin transparent plates at all depend upon the extent of these fits; but will to sense be the same, whether the extent of the fit of reflexion be  $\frac{1}{2}$ , or  $\frac{1}{4}$ , or  $\frac{1}{8}$ , or  $\frac{1}{16}$  part of the interval between the fits, as I had before taken notice in my *Reply*<sup>y</sup>. For if the reflexion be most copious at the line 2 K, and decrease from thence both ways, as Sir *Isaac Newton* teaches; from this alone it will follow, that the phænomena must be the

<sup>w</sup> Reply, pag. 25.  
<sup>y</sup> Pag. 26, 29.

<sup>x</sup> Full Confutation, pag. 25.

the same as he has observed, whatever be the extent of the fit of reflexion.

Having now sufficiently examined this *sufficient quotation*, with the several inferences this gentleman has drawn from it, it will not be beside the matter in hand to make a few remarks.

*Remark 1.* Had Mr. *Robins* been able to shew, not only by a disputable inference drawn from any passage of the *Opticks*, but by the plain and express words of Sir *Isaac Newton*, that this Great Man had *indubitably supposed* the extent of the fit of reflexion to be equal to that of transmission; yet unless he could also shew, that this equality was deducible from any of the preceding *Observations*, it must be looked upon as a mere hypothesis only. And a mere hypothesis, though of Sir *Isaac Newton* himself, is no foundation to build upon in the true method of philosophising.

*Remark 2.* From supposing the extent of the fit of reflexion always equal to that of transmission, it will follow, that in some cases much the greater part of the homogeneous rays will be transmitted, when they are all, according to Mr. *Robins*, in the fit of reflexion. For instance, in viewing the thin plate of air by reflexion, the beginning of black is placed at the thickness 2, in Sir *Isaac Newton's* table page 206, which thickness by computation is a little greater than A 1 in the 6th figure. Therefore at this thickness 2 there is no sensible colour seen by reflexion, and consequently much the greater part of the rays of the extream violet are transmitted, which yet according to Mr. *Robins* are all in the fit of reflexion at that thickness. And this, if it be not a downright absurdity, yet renders the consideration of this extent of the fits, and indeed of the fits themselves, utterly useless.

*Remark 3.* From the same supposition it also follows, that part of the rays will be reflected,

when they are all in the fit of transmission. And this will happen, not only near the extremity of the extent of the fit, as in the second remark, but near the middle of it. For instance, some rays of the extreme violet will be reflected, not only at a thickness something less than A 1, (where Sir *Isaac Newton* tells us, we *must not conceive* the colour to be *precisely limited*, but to *decay indefinitely*) but at one half, nay at one quarter of that thickness: which may be easily seen from the several thicknesses, at which the beginning of black, black, and very black are placed in Sir *Isaac Newton's* table, pag. 206.

*Remark 4.* Sir *Isaac Newton*, though for want of observations he assigned no proportion between the extent of the fit of reflexion and that of transmission, yet was aware that, in the common transparent substances, the former of these was considerably less than the latter.

This instance of the sagacity of our Great Philosopher plainly appears from the explanation of the 13th proposition of his second book, where he uses these words.

“ If the thickness of the body be much less  
 “ than the interval of the fits of easy reflexion and  
 “ transmission of the rays, the body loseth its re-  
 “ flecting power. For if the rays, which at their  
 “ entering into the body are put into fits of easy  
 “ transmission, arrive at the farthest surface of the  
 “ body before they be out of those fits, they must  
 “ be transmitted.”

Here by teaching us, that, if the thickness of the body be *much less* than the interval of the fits of reflexion and transmission, the rays, when they arrive at the farthest surface of the body, will not be out of the fit of transmission, and therefore must be transmitted, he plainly intimates, that, if the thickness be but *a little less* than that interval, the rays, when they arrive at the farthest surface, will



be out of the fit of transmission, and entered into the fit of reflexion, and therefore will not be transmitted, but reflected. Can any thing more plainly shew, that he supposed the fit of transmission to take up the greater part of the interval, and the fit of reflexion to take up the lesser part of the same interval?

But had he supposed the two fits to be equal in extent, as these his expositors so weakly surmise, how comes he to use the expression *much less than* the interval? The most careless writer, with that supposition in his head, could hardly have avoided saying *less than half* the interval.

That this is no careless or accidental expression, those, who have attended to Sir *Isaac Newton's* manner of writing, need not be told: and others, who want either the capacity, or the attention, to perceive the accuracy and circumspection with which he always writes, may learn the same thing from his using these very words *much less*, upon the like occasion, in another place. For in the very passage Mr. *Robins* produces to shew, that Sir *Isaac* indubitably supposed the extent of the two fits to be equal to each other, are these words. “ Its transmission thro’ the first surface is at the distance 0, “ and it is transmitted thro’ both together, if their “ distance be infinitely little, or *much less* than 1.” Where 1 is the interval between the two fits, and if such a writer as Sir *Isaac Newton* had indubitably supposed the two fits to be equal in extent, instead of saying *much less than* 1, he would indubitably have said *less than* 1.

But this is not all. For the honour of our Great Master, as well as for the information of these self-sufficient gentlemen, who have the modesty to set up for the only judges and interpreters of his writings, I must beg leave to go a little farther. I shall not content my self with having shown, upon

how slender grounds they have fathered their own shallow conceptions upon Sir *Isaac Newton*; but shall *irrefragably prove*, that his doctrine is directly contrary to what they represent it. With this view I shall here lay down and make good three capital points of his Theory, in every one of which, after all the airs they give themselves, I find them scandalously ignorant.

*Point 1.* When any ray of light falls upon a refracting surface, if the ray be reflected, it is in a fit of reflexion: And if the ray be transmitted, it is in a fit of transmission.

*Point 2.* If the ray be in a fit of reflexion, it is actually reflected: And if it be in a fit of transmission, it is actually transmitted.

*Point 3.* The middle of the fit of reflexion in every ray of homogeneal light is not at the same distance from the surface the light enters.

1. The first of these points, that, when any ray of light falls upon a refracting surface, if the ray be reflected, it is in a fit of reflexion; and if the ray be transmitted, it is in a fit of transmission, is the first and fundamental principle of the doctrine of the fits of reflexion and transmission. If this be false, the whole falls to the ground.

Here I would ask Mr. *Robins* and his associates, whether this principle be true or false. If they allow it to be true, then from this I will beyond contradiction demonstrate the truth of the second point, which he has again and again declared to be an *erroneous position*.

Perhaps therefore they will tell me, it is false. If so, I must again ask these thinking gentlemen, How then was it possible for Sir *Isaac Newton* to discover these fits of reflexion and transmission, or to know any thing at all of their existence? Is it not from observation only, from his actually seeing the rays

rays reflected in one place, and transmitted in another, that he concludes them to have these alternate fits of reflexion and transmission? But if rays can be reflected, when they are not in a fit of reflexion, what right has he to conclude, that where he sees them reflected, there they are in a fit of reflexion? And if they can be transmitted, when they are not in a fit of transmission, how can he justly conclude, that where he sees them transmitted, there they are in a fit of transmission?

But this matter is so plain, I am ashamed of having said so much about it. I shall therefore proceed to the second point.

2. If the ray be in a fit of reflexion, it is actually reflected: and if it be in a fit of transmission, it is actually transmitted.

This is easily proved. For all the rays, which fall upon a refracting surface, are either reflected or transmitted. And since by point 1, every ray, which is transmitted, is in a fit of transmission, no ray is transmitted, when in a fit reflexion. Therefore every ray, which is in a fit of reflexion, is reflected.

And since by point 1, every ray, which is reflected, is in a fit of reflexion, no ray is reflected, when in a fit of transmission. Therefore every ray, which is in a fit of transmission, is transmitted.

Here it is hardly worth while to take notice of that small portion of the light, which, in falling upon a refracting surface, may neither be reflected nor transmitted, but stifled and lost by repeated reflexions within the transparent substance, this, if any, being exceedingly small in the common transparent substances, unless when of a great thickness, and our dispute not being affected by it.

But this second point so often occurs in the controversy between Mr. *Robins* and me, that however clear and satisfactory be the demonstration above given,



given, it may not be amiss to prove it in another manner, from the plain and express words of Sir *Isaac Newton*.

His 13th Proposition runs thus. “ The reason  
 “ why the surfaces of all thick transparent bodies  
 “ reflect *part of the light* incident on them, and re-  
 “ fract the rest, is, that *some rays* at their incidence  
 “ are in fits of easy reflexion, and others in fits of  
 “ easy transmission.”

And in the explanation of that proposition he has these words.

“ If the rays, which at their entering into the  
 “ body are put into fits of easy transmission, ar-  
 “ rive at the farthest surface of the body before  
 “ they be out of those fits ” (What then? Pray,  
 Sir, mind the expression) “ they MUST be trans-  
 “ mitted.”

Also, in laying down the use of the 18th and the preceding propositions, he tells us. “ From  
 “ these propositions it is easy to collect the inter-  
 “ vals of the fits of easy reflexion and easy trans-  
 “ mission of any sort of rays — and thence to  
 “ know, whether the rays SHALL be reflected or  
 “ transmitted, &c.”

I could, upon occasion, produce you half a dozen more passages to the same purpose: but these three may suffice to shew it is the doctrine of Sir *Isaac Newton*, that when rays are in a fit of reflexion, or of transmission, at their meeting with a refracting surface, they are accordingly reflected, or transmitted.

But what will you say, Sir, if I should prove this to be the doctrine of Sir *Isaac Newton*, from Mr. *Robins*’s own writings. After so much as he has said to the contrary, this will be called a *hardy* undertaking, and yet I hope to succeed.

That gentleman in his *Remarks*, pag. 104, 105. tells us, Sir *Isaac Newton* concludes, *that the light,*  
*which*

*which is reflected, is only such, as is in a fit of reflection.* A reference here would have been of service, in order to let us know, whether Sir *Isaac Newton* concludes this in so many words, or it be a conclusion made for him by Mr. *Robins*. But not finding that these words are any where used in the book of *Opticks*, I must take them to be Mr. *Robins*'s own words. I ask therefore, what reason he has to impute this conclusion to Sir *Isaac Newton*. He will answer perhaps, that this is not only agreeable to the constant tenour of Sir *Isaac*'s doctrine, but that particularly in his 12th and 13th proposition, where he first establishes the existence of these fits, from seeing the rays actually reflected at certain thickneses, he concludes that at these thickneses they are in fits of reflexion.

Now if this be his answer, I am very ready to allow of it. But then I must take leave to ask another question. Does not Sir *Isaac* in the very same propositions, from seeing the rays actually transmitted at certain thickneses, conclude that at those thickneses they are in fits of transmission? And if so, may I not say he concludes, *that the light, which is transmitted, is only such, as is in a fit of transmission?* Surely, I have the same right to say the one, as Mr. *Robins* has to say the other.

Now, *if the light, which is reflected, is only such, as is in a fit of reflection*, then no light is reflected when in a fit of transmission.

And *if the light, which is transmitted, is only such as is in a fit of transmission*, no light is transmitted when in a fit of reflexion.

And as all the incident light is either reflected or transmitted, it follows that all the light, which is in the fit of reflexion is reflected; and all the light, which is in the fit of transmission is transmitted.

Again, in the *Full Confutation*, pag. 24. when he calls the lines 1 I, 3 L, *the limits, within which*

*which rays may be reflected*, are we not to understand, that without these limits rays cannot be reflected? And when he calls the lines 3 L, 5 M, *the limits, within which rays may be transmitted*, does he not mean, that without these limits rays cannot be transmitted?

If so, the rays cannot be transmitted, but must all be reflected *within the limits* 1 I, 3 L, that is according to Mr. Robins, *within the limits* of the fit of reflexion: and the rays cannot be reflected, but must all be transmitted, *within the limits* 3 L, 5 M, that is according to Mr. Robins, within the limits of the fit of transmission.

Also, in the same *Full Confutation*, pag. 28. he affirms, that *when the light after entering the first surface of a body of any thickness arrives at the second, — near half the species of light will be wholly transmitted*. Here, if you ask him, how he knows this *will* be so, he can only tell you, it is because *near half the species of light will be* in the fit of transmission: Now, if the rays, which are in the fit of transmission, *will be wholly transmitted*, by parity of reasoning from Sir Isaac Newton's observations, the rays, which are in the fit of reflexion, will be wholly reflected.

To all this some persons may object, that in other parts of Mr. Robins's writings he appears of a different opinion, nay, in some passages, which I myself have quoted from him, he maintains the direct contrary. But if this way of arguing be allowed of, it will be hard to prove, that this gentleman has any opinions at all. For, as his views are seldom very extensive, he often asserts a thing to get over a present difficulty, which he finds himself obliged to contradict, when it comes to be urged against him. His arguments, like the *Pelias basta*, are *irrefragable* and *irresistible*, when managed by himself, but of no manner of force in any other hand:



hand : and his assertions are so far from being of the number of the *veritates æternæ*, that they are truths to-day and falsehoods to-morrow, unquestionably true in one page and absolutely false in the next.

This point being now so fully cleared, it is hardly worth while to take notice of what Mr. *Robins* alledges against it, which yet, because I would not omit any of his arguments, how weak soever, I shall here consider.

“ Sir *Isaac Newton*, says he, plainly distinguishes between a ray’s being in a fit of reflexion, and its being actually reflected, in his 14th proposition of the second book, which is thus laid down, *Those surfaces of transparent bodies, which, if the ray be in a fit of refraction, do refract it most strongly, if the ray be in a fit of reflexion, do reflect it most easily.* Here it is evidently supposed, that a ray may be in a fit of reflexion, and yet not be in fact reflected ; for if every ray, which is in a fit of reflexion at any surface, be certainly reflected, no one surface can be said to reflect the rays, when in a fit of reflexion, more easily than another.”

I believe, Sir, you will think, this is very far from being *plain* or *evident*. For if two different surfaces do each of them reflect a ray of the same species, that surface may justly be said to reflect its ray *most easily*, which reflects it in a part of its interval where it is less disposed to be reflected, or which reflects it at a less obliquity. It is much Mr. *Robins* should not think of this ; for in the proof of the very proposition he quotes, Sir *Isaac Newton* refers to the first proposition, and in the proof of that he has these words, “ Superficies therefore which refract most do soonest reflect all the light which is incident on them, and so must be allowed most strongly reflexive.” He  
is

is treating here of different superficies, each of which reflect all the light incident upon them, but one does it at a less obliquity than the other. And that superficies, which reflects the whole light at the lesser obliquity, as it is here called *most strongly reflexive*, is in the 14th proposition said to *reflect the ray most easily*. But it is time to come to our third point.

3. The middle of the fit of reflexion in every ray of homogeneous light is not at the same distance from the surface the light enters.

When light is transmitted through the surface AH in Sir *Isaac Newton's* 6th figure, the middle of the fit of reflexion in the greater part of the rays of the extreme violet falls out at the thickness A 2 : but that this is not the case of the whole of those rays, is easily proved.

For in my 3d remark it is shown, that some of these rays are reflected at a thickness equal to one half, and some at a thickness equal to one quarter of A 1 ; and consequently, by the first point above laid down, they are in the fit of reflexion at those thicknesses. But rays, which are in the fit of reflexion at the thickness half A 1, must be in the middle of that fit, at the thickness  $\frac{1}{4}$  of A 2 or less; and those, which are in the fit of reflexion at  $\frac{1}{4}$  of A 1, must be in the middle of that fit at  $\frac{1}{8}$  of A 2 or less.

Mr. *Robins* was therefore much mistaken, when he said, he “ knew, <sup>a</sup> that Sir *Isaac Newton* supposed the middle of the fits of reflection of every “ ray of the same species to be at the same distances from the surface the light enters,” and all he has drawn from this principle, either in favour of himself<sup>b</sup>, or against me<sup>c</sup>, falls to the ground.

Here, Sir, I must observe, that although I had not been able thus clearly to make out, that what I have

<sup>a</sup> Full Conf. pag. 25.

<sup>b</sup> Ibid. pag. 25.

<sup>c</sup> Ibid. pag. 25, 26.

I have advanced in relation to these three points, and particularly the two last of them, upon which so much of our controversy turns, is agreeable to the sentiments of Sir *Isaac Newton*, it could not have been of the least advantage to Mr. *Robins*. For if, as he says, <sup>d</sup> *I have taken upon me to improve upon Sir Isaac Newton*, he is not to look for those improvements in the book of *Opticks*, nor, if he does not find them there, is that a sufficient reason for condemning them. But farther, if I have not only treated more explicitly of the fits of transmission and reflexion in some particulars, than has been done by Sir *Isaac Newton*; but have considered them in a way different from his, and have applied them to different purposes, it would not be sufficient to condemn any thing I lay down, to shew that it is different from, or even contrary to what is laid down by him: for that may be true in the way I consider them, which in his would be false.

To make this be the better understood, it will be necessary to distinguish between the doctrine laid down by Sir *Isaac Newton*, and the attempt I have made to carry that doctrine a little farther.

What Sir *Isaac Newton* expressly teaches concerning these fits, is only how to find the intervals between them, or rather between the middle of the fits, in any medium the rays are passing through. For as to the extent of either fit, he is so far from expressly assigning it, that he never mentions it.

But I was led by my subject to consider not only the interval between the fits, but also in what proportion that interval was divided between each of the fits, that is, what was the extent of the fit of reflexion and of the fit of transmission.

When light is passing through a given medium, and the interval of the fits is determined, Sir *Isaac*  
treats

<sup>d</sup> Remarks, pag. 104.



treats of the disposition to be reflected or transmitted, absolutely, as it is in the rays themselves; without any limitation of it in regard to any second medium, upon whose surface the rays are next to fall.

I on the other hand consider this disposition not absolutely, as it is in the rays themselves alone considered, but relatively, with regard to the next medium, and determine the extent of it accordingly.

The reason of this is, that the interval between the fits depends only upon the medium through which the light passes, but the extent of the fits depends upon the medium, on whose surface the light is next to fall.

Therefore a ray, which in Sir *Isaac Newton's* doctrine is said in general to be in a fit of reflexion or of transmission, may, in my way of considering these fits, be in a fit of reflexion with respect to one medium it may next meet with, and in a fit of transmission with regard to another. For I never consider a ray as in a fit of reflexion, but where it would be actually reflected from the surface of the next medium, nor in a fit of transmission, but where it would be actually transmitted. And to do otherwise would be highly absurd. For if a ray, in passing through one medium, do arrive at the surface of a new medium, and at its incidence thereon be actually disposed to be reflected by that surface, what can hinder it from being actually reflected?

From this peculiar way of considering the fit of reflexion and transmission, I had in my *Essay* drawn some deductions, which I made no parade of as new discoveries, but laid them down as part of the doctrine of Sir *Isaac Newton*, from which indeed it was not difficult to derive them: and the modesty of this proceeding, being, I suppose, *surprising to Mr. Robins*, might at first lead him and his friends into

a mistake. Upon looking for those things in the book of *Opticks*, which were not to be found there, though with a little attention they might be deduced from the doctrine there delivered, the next step was to condemn them as erroneous.

This occasioned my laying down those deductions somewhat more explicitly, in the form of distinct propositions, in my *Reply*: and from these, particularly from my 6th proposition, it was now, if not before, very easy to see, that I considered the disposition of the rays of light to be reflected or transmitted, in a different manner from Sir *Isaac Newton*.

But if neither those propositions, nor all I have now said, be satisfactory to Mr. *Robins*, I must have recourse to that right, which has always been allowed to writers who advance any new speculations, I mean that of defining any terms they have occasion to make use of: And being the first person, as far as I know at least, that has ever treated of the dispositions of the rays of light to be reflected or transmitted, relatively, with regard to the next medium, whose surface they may fall upon, I shall take the liberty of laying down the following definitions.

#### DEFINITION 1.

When a ray of light, in its passage through any medium, falls upon the surface of a new medium, and is thereby reflected, the ray, at the instant of its incidence, is in a disposition to be reflected from that surface. And, *vice versa*, if the ray be in a disposition to be reflected by that surface, and do actually fall upon it, it is actually reflected.

#### DEFINITION 2.

When a ray of light is in a disposition to be reflected by any refracting surface, I say, it is in a fit of reflexion with regard to that surface.

## DEFINITION 3.

If a part of the interval between two successive fits of transmission in any ray, be so taken, that in every point thereof the ray is in its fit of reflexion with regard to any refracting surface, such part of the interval is called the extent of the fit of reflexion in that ray with regard to that surface.

*Corollary.* A ray of light will be reflected by a refracting surface, that meets it in any point of the extent of its fit of reflexion.

Now if Mr. *Robins* is pleased to look over my *Essay* and *Reply* a second time, he will find, that all I have there said about the fits of reflexion and transmission, is perfectly agreeable to the sense of these definitions. He will perhaps tell me, I ought therefore to have laid them down sooner. But as to that, I was at liberty to do as I thought fit. I hope I have from the first so expressed my self, that no attentive and fair reader could have the least doubt of my meaning, and I chose, as I said before, to make no parade, nor to give my self any airs of new discoveries.

In the last paragraph of page 25 this gentleman seems to make me a very liberal concession. He says, “ in all common objects, where the light incident upon them has passed for a great length through the air, the extent of the fit of reflexion ought to bear that proportion to the whole interval between the fits, which Dr. Jurin has assigned.” And presently after he tells us, “ this supposition” (that all the light in a fit of reflexion is actually reflected) “ accounts for the quantity of light reflected in common from objects.”

Now this being the only concession Mr. *Robins* has made me in the whole controversy, it is great pity the reason he gives for it, should be so very weak.



weak. That reason is, that, “ when light has  
 “ passed through any great number of fits of re-  
 “ flection and transmission — the spaces 1 I L 3,  
 “ 5 M O 7, 9 P R 11, &c. in which Sir *Isaac*  
 “ *Newton* represents the rays of light as in fits of  
 “ reflection, must be contracted in breadth in pro-  
 “ portion to the quantity of light reflected.” But,  
*when light has passed through any great number of fits,*  
*will not the spaces 3 L M 5, 7 O P 9, 11 R S 13,*  
*&c. in which Sir Isaac Newton represents the rays of*  
*light as in fits of transmission, also be contracted in*  
*breadth in the very same proportion with the former*  
*spaces? If so, the proportion between the extent*  
*of these two fits will be the same as at first, and*  
*consequently, I can reap no advantage from this so*  
*kind and bountiful a concession.*

If herein I have made any mistake, I am ready to ask this gentleman pardon: for such is the sublimity of this paragraph, that I am not sure I rightly comprehend it.

In page 27 this gentleman objects to my having assumed in my 10th and 11th proposition, (he means in the corollaries to the 11th proposition) that *transparent substances illuminated by the common light of the day indifferently transmit or reflect all the species of light, not one more than another.* “ Now,” says he, “ this is not the case of such surfaces; “ for, when the light having passed through the “ air falls upon any transparent body, or after entering the first surface of a body of any thickness arrives at the second, according to Sir *Isaac Newton*’s doctrine, as represented in the 6th figure of the second book of his *Opticks*, near half “ the species of light will be wholly transmitted.”

Here I must first ask this accurate writer, what reason he has to say, *near half the species of light will be wholly transmitted.* Will he say this happens,

because the extent of the fit of transmission is nearly equal to that of the fit of reflexion? This would be begging the question, it being what I have always denied.

But were we to allow, the extent of these two fits to be nearly equal, and consequently that near half the species of rays are in the fit of transmission, what reason has he to say, that near half the species will be wholly transmitted? To say, all the light, that is in a fit of transmission, is transmitted, and *all the light, that is in a fit of reflexion, is reflected*, is with him an *erroneous position*.

Lastly, does not he see, that he is here comparing together two cases widely different? When light, being transmitted through the first surface of a medium, as represented in Sir *Isaac Newton's* 6th figure, is at that surface, all of it as he pretends, or most of it as I allow, put into the middle of the fit of transmission, and after a long passage arrives at a second surface parallel to the first, every ray will have passed just the same distance through the medium; and consequently, all or the greater part of the rays of the same species will at that second surface be exactly in the same part of their fit whether of reflexion or transmission. But in the case I consider, where light either emitted from the sun, or reflected from the clouds, after passing through the air, falls upon the plane surface of a transparent body, some rays must have come from a remoter point of the sun or clouds, than other rays have come from. And although, in favour of his objection, we were to suppose, that all rays of the same species were exactly in the same point of their fit at their emission from the sun or reflexion from the clouds, yet by reason of the infinite diversity in the distances they pass through, as many of them must be in any one point of the interval between the  
the

the fits, as in any other point, when they arrive at the plane surface of the transparent body. Here therefore as many rays of one species, as of any other species, may be in the fit of reflexion, though it cannot be so in the case represented by Sir *Isaac Newton's* figure. So that my supposing, that colourless transparent substances may indifferently reflect or transmit all the species of light, not one more than another, is not <sup>e</sup> *irreconcilable with Sir Isaac Newton's deductions from the phænomena.*

Much the same answer may suffice for the argument, by which he endeavours to prove, that <sup>f</sup> *I have made my hypothesis even inconsistent with it self.*

In order to make this out, he first lays down my assertion, that when the rays of any species, as the extreme violet, are transmitted through the surface of a transparent substance, the greater part of them will there be put into the middle of the fit of transmission, or very near it. And from this he infers, that upon meeting with a new refracting surface at the distance denoted by any one of the lines 2 K, 6 N, 10 Q, 14 T, &c. the greater part of those rays will be in the middle, or very near the middle of a fit of reflection, when they fall on that surface. "And," says he, "this will always be the case of some species, nay in all common objects, upon which the light falls, it will happen to very many. This is a direct contradiction to the first assertion abovesited from the 9th proposition of homogeneous light in general, that as many rays will meet the surface, upon which they are incident, in any one point of the interval between the fits, as in any other."

Here, in order to make out this *direct contradiction*, Mr. *Robins* again takes it for granted, that

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what

<sup>e</sup> Full Confutation, pag. 27.

<sup>f</sup> Ibid. pag. 28.



what happens in the case represented by Sir Isaac Newton's 6th figure, must happen in all common objects, upon which the light falls. But we have just now shown, that the case represented in that figure is widely different from the case of common objects illuminated by the light either emitted from the sun, or reflected from the clouds. Therefore this gentleman has not proved my *hypothesis* inconsistent with it self, any more than he had before proved it irreconcilable with Sir Isaac Newton's deductions from the *phænomena*: nor has he in the least made out, that as soon as I came to apply my *hypothesis* to the *phænomena*, I found myself obliged to contradict the foundation of my scheme, and thereby have myself absolutely subverted it.

I come next to consider those two <sup>s</sup> assertions, made, as Mr. Robins thinks, in the spirit of the loosest coiners of hypotheses, who think themselves at liberty to multiply suppositions, just as shall be thought expedient for the support of a favourite scheme, without troubling themselves in the least about any connection between them.

Now, Sir, the practice here censured being what I have always held in high contempt, as I have more than once shown in my writings, I had taken some care not only to avoid that practice, but even to prevent the suspicion of it. Before I came to lay down those propositions in my *Reply*, which contain the deductions I had drawn from Sir Isaac Newton's doctrine, I advertised my reader, that <sup>h</sup> the truth of them would easily appear to those, who had carefully perused Sir Isaac Newton's observations. And when I laid down the first of those two loose hypotheses, <sup>i</sup> that the rays of one species, which are transmitted through the transparent surface at *A*, cannot  
all

<sup>s</sup> Ibid.

<sup>h</sup> Reply, pag. 16.

<sup>i</sup> Ibid. pag. 26.

*all be in the middle, or be put into the middle of the fit of easy transmission at that point A: but some of them must be past the middle of the fit, and others of them not arrived at the middle of the fit; though not a word be there said to shew, how this may happen, yet, one would think, a reader of common penetration might have been able to deduce it from my 9th proposition, which had been already laid down. From the principle made use of in that proposition, that as many rays will meet the plane surface in any one point of the interval as in any other point, it is plain they cannot all be in the middle of the fit. And, whatever be the cause, that puts the rays into new fits upon their arrival at the plane surface, it would be utterly absurd to imagine, that rays, which were in all possible different parts of their old fit, should by any cause be put all at once into the very middle of the new.*

*As to the other of these loose hypotheses, that<sup>k</sup> the greater part of the rays will there be put either into the middle of the fit, or very near it, it is no other than a deduction from what I had in the preceding page laid down from Sir Isaac Newton, that the rays are most copiously reflected at the thickness A 2. For, if the greater part of the rays are reflected at the thickness A 2, the point 2 must be the middle of the fit of reflexion, and consequently the point A must be the middle of the fit of transmission, of the greater part of the rays: And this, one would think, should have been no difficult matter to make out, for persons, who had carefully perused Sir Isaac Newton's observations.*

Here, Sir, before I proceed farther, I must beg leave a little to recapitulate.

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From

<sup>k</sup> Ibid. pag. 27.

From an experiment laid down in my *Essay*<sup>1</sup> I had inferred, that in the common transparent substances the extent of the fit of reflexion is much less than that of transmission. On the contrary Mr. *Robins* did in his *Remarks*<sup>m</sup> assert, and that in the most dogmatical and magisterial manner possible, that these two extents were equal, for that this equality *was necessary towards producing the appearances* of thin plates recorded by Sir *Isaac Newton*.

In my *Reply*<sup>n</sup> I shewed this gentleman how those appearances might be accounted for without supposing such equality.

To my method of accounting for those appearances, Mr. *Robins* in his *Full Confutation*, has made the following objections.

1. Whereas I suppose, that rays of one and the same species are not all in the middle of the fit of reflexion at the same distance from the surface they have been transmitted through; Mr. *Robins* maintains,<sup>o</sup> *that all the rays of the same species are in their fits of reflexion at the same distances from the surface they have entered*: and he calls this a *principle of Sir Isaac Newton*, though he does not pretend to say, that Sir *Isaac Newton* has any where laid down this principle, but only endeavours to infer it from a disputable proposition.

In answer to this I have proved, that no such inference can be justly drawn from this proposition, and that Sir *Isaac Newton* holds no such principle, but the contrary.

2. Whereas I suppose, that every ray, when in a fit of reflection, is actually reflected upon meeting with a refracting surface, Mr. *Robins* maintains<sup>p</sup>,  
that

<sup>1</sup> Art. 224.

<sup>m</sup> Pag. 105, 106, 107, 111.

<sup>n</sup> Pag. 26, 27, 28.

<sup>o</sup> Full Confutation, pag. 26.

<sup>p</sup> Ibid. pag. 26, 27.



that Sir Isaac Newton plainly distinguishes between a ray's being in a fit of reflexion, and its being actually reflected.

I have shown him, that this distinction is a vain and groundless surmise of his own, and have proved from Sir Isaac Newton's express words, that the reason why *part of the light* is reflected, is that *some rays* are in fits of easy reflexion; that when rays are in the fit of transmission, *they MUST be transmitted*; and that from the intervals of the fits we may know whether the rays *shall* be reflected or transmitted. I have moreover observed to him, that if Sir Isaac Newton had made this distinction, yet I have not made it; and if he will argue against me, he must use my words in the sense I myself use them in; and that I never speak of the fit of reflexion, but where the ray will be actually reflected, if it meet a refracting surface.

3. That my hypothesis <sup>a</sup> is irreconcilable with Sir Isaac Newton's deductions from the *phænomena*.

I have shown, that the means he uses to prove this, is to compare together two cases, which are widely different.

4. That my <sup>r</sup> hypothesis is inconsistent with it self.

I have shown, that he has no other proof of this neither, than by comparing together two cases that have no resemblance.

Therefore, notwithstanding any thing Mr. Robins has yet produced, the *phænomena* of thin plates seem to be fairly accounted for, though the extent of the fit of reflexion be much less than that of the fit of transmission.

Having shown upon how little reason, this arbitrary censor has passed sentence upon what he calls my hypothesis, it may not be amiss a little to consider

<sup>a</sup> Ibid. pag. 27.

<sup>r</sup> Ibid. pag. 28,

consider the hypotheſis he himſelf lays down in the room of it.

He tells us\*, “the ſame phænomena may be  
 “ more eaſily accounted for, only by ſuppoſing,  
 “ that tho’ every ray of the ſame ſpecies be al-  
 “ ternately diſpoſed to reflection and tranſmiſſion  
 “ at the ſame diſtances from the ſurface, it en-  
 “ ters ; yet, that all the rays of the ſame ſpecies  
 “ ſucceeding each other may not receive their diſ-  
 “ poſitions to reflexion equally ſtrong.” This is  
 illuſtrated by the following curious note at the  
 bottom of the page. “ For example, if the rays  
 “ of light are put into theſe alternate diſpoſitions  
 “ by exciting at the refracting ſurface a vibra-  
 “ tory motion *in the refracting medium*, as Sir *Iſaac*  
 “ *Newton* propoſes ; it is not difficult to conceive,  
 “ how the vibrations, that are excited by the  
 “ different rays of the ſame ſpecies, which enter  
 “ the ſurface in ſucceſſion, may *in ſome of the*  
 “ *rays* be more vigorous than *in others*.”

Now to me it is ſo *difficult to conceive*, whether  
 this *vibratory motion*, or theſe *vibrations*, exiſt in  
 the *refracting medium*, or in the *rays* themſelves, or  
 in both together, that I profeſs my head turns  
 round with the contemplation. For which reaſon  
 I muſt beg to be excuſed from proſecuting this un-  
 laboured hypotheſis any farther ; but ſhall however  
 examine how far thus much of it is agreeable to Sir  
*Iſaac Newton*’s doctrine, or conſiſtent with it ſelf.

Sir *Iſaac Newton* throughout his book of *Opticks*  
 teaches, that rays of the ſame ſpecies are perfectly  
 alike in colour, in the degree of refrangibility and  
 reflexibility, in the intervals of their fits, and in  
 every other circumſtance as far as he could diſcover  
 by experiment and obſervation.

But

\* Ibid. pag. 30.

But this ingenious person supposes, *that all the rays of the same species may not receive their dispositions to reflexion equally strong.* If so, some rays of the same species must be more reflexible than others, contrary to Sir *Isaac Newton's* doctrine.

Again, a difference in the disposition of the rays to reflexion must arise from a difference in the vibrations of the medium; and a difference in the vibrations of the medium must be attended with different intervals between the fits of transmission and reflexion. Therefore, by this hypothesis of Mr. *Robins*, rays of the same species must have different intervals between their fits. But by Sir *Isaac Newton's* doctrine, rays of the same species have the same interval: and this very doctrine of Sir *Isaac Newton* is laid down by Mr. *Robins* in the same sentence with his own hypothesis. Perhaps his two Doctors put it in by way of *corrective*.

This hypothesis therefore of Mr. *Robins* being neither agreeable to Sir *Isaac Newton's* doctrine, nor so much as consistent with itself, it will be to little purpose to enquire how *easily* it will *account for the phenomena*. I shall only take notice, that in this hypothesis, and the solution built upon it, no mention is once made of the equality between the extent of the fit of reflexion and that of transmission. And here, Sir, after the cruel mortification I have suffered, for supposing these two extents to be unequal, (Those, who have denied an equality of another nature, have hardly undergone more persecution) I leave it to you to judge what a consolation it must be to me, that by means of this ingenious hypothesis, *the phenomena can be so easily accounted for*, without supposing that equality. Surely, *the thought could never have been entertained but by one of the highest degree of skill in the subject.*



In my *Reply*, pag. 31. I had taken notice, that Sir *Isaac Newton* constantly numbers the series or orders of colours from the black spot outwards: but that Mr. *Robins*, without giving any notice of his intention, is pleased to number them the contrary way. Now I took that notice, because I had been myself so far misled by this proceeding, that I had once drawn up an answer to that gentleman upon the supposition, that he numbred those series or orders the same way with Sir *Isaac Newton*; and I was apprehensive, that every reader must likewise be misled by it. Upon which Mr. *Robins*, in the height of his indignation at the *dull pedantick censure*, and *puerile reflections* I had used upon this occasion, is pleased to say, ' *both these assertions are absolutely false*. These are strong words; but to make amends, the proof is as weak as one could wish. In order to shew, that *Isaac Newton* does not constantly number the series or orders of colours from the black spot outwards, he says, " Sir *Isaac Newton* in the second book, *Obs.* 18. has described the colours in the same manner I have done, calling each colour 1st, 2d, 3d, 4th, &c. according to the order in which they arose on the bubble; and contrary to the order in which they are number'd from the black spot outwards." It is true, that the single *colours* are thus number'd in that observation; but the question between us is, whether *the series or orders* of colours are so number'd: And these are so far from being thus number'd, either in that or any other observation, that Sir *Isaac Newton* never so much as mentions the words *series or orders* of colours, till the observations being all over in the first part, he comes in the second part of the book to write his *Remarks upon the fore-*

\* Full Confutation, pag. 31:

*foregoing observations*, where every time he mentions the word *series*, or *orders*, he numbers them from the black spot outwards, as I had before asserted. Now, Sir, what are we to think of this proceeding? Can it arise from ignorance, or inadvertency? No, that cannot be. By producing this evidence, shallow as it is, from the 18th observation, this gentleman appears to have made the most diligent search for proof against me: and it is impossible to make any search, without seeing that to be unquestionably true, which he avers to be *absolutely false*.

His behaviour in regard to my second assertion, is, if possible, still worse: he now commits suicide, whereas before he had only murdered Sir *Isaac Newton*. “ I had, says he, in the beginning of “ the passage here referred to given notice, that I “ also reckoned them,” (the series or orders of colours) “ as they arose on the bubble, and had added this reason for my doing so, &c.”

Now *the beginning of that passage* runs thus, “ The “ colours, which arose successively upon the bubble of water, Sir *Isaac Newton* has described very “ distinctly, with all the concomitant circumstances; but were it examined, &c.” In which words I can find no mention of *series* or *orders*, nor any notice given how they are to be reckoned, nor any reason added for the doing so, though I have again and again looked them over with *careful attention*, and with my best pair of spectacles. I must therefore take the liberty of saying, that the second assertion also, which Mr. *Robins* affirms to be *absolutely false*, is *indubitably true*.

To how much shame may an obstinate writer expose himself, by defending an oversight he ought to acknowledge! To hide a spot in his coat, he plunges into *Fleetditch*.

So hideous a spectacle may well make me bless myself, that when I fell into any mistake, I had the grace to act in another manner.

Apprehending that Sir *Isaac Newton* supposed the light of the white ring in the first order of colours to be *equal, or nearly equal* to the light of white metals, I had at the end of my *Essay* dropped a suspicion of some mistake. But in my *Reply* to Mr. *Robins's Remarks*, I have acknowledged, that I had misapprehended Sir *Isaac Newton*, and have thereupon declared that *my difficulty was at an end.*<sup>u</sup>

Now what would this gentleman require more? Would he have me stand in a white sheet? Must I mount the stool of repentance, and bewail my crime in the face of the congregation? It was but a suspicion at most, and was accompanied with no insolence, no over-bearing, no airs of superiority, but was expressed in so modest and respectful a manner, that *how ill soever it might appear to be grounded, I had reason to hope, as I said in my REPLY<sup>x</sup>, it would easily be pardoned by candid judges.* Here then was an opportunity for our judge to shew his candour, if he had any. And if he had none, should he not treat me with common justice? How comes it, that in quoting my *Reply* he leaves out the word *nearly*? My expression was,<sup>y</sup> *as strong, or nearly as strong,* <sup>z</sup> *equal, or nearly equal.* Not content with my acknowledgment, he admits of no excuse, will not so much as allow me the benefit of this mollifying term *nearly*, but, as if he were attorney general to *Rhadamanthus*, he studies to *exaggerate* the over-sight all he can, and pursues me without mercy for two pages together. Why what a remorseless and unrelenting mind is here! Confess, and be hanged,

<sup>u</sup> Reply, pag. 34.

<sup>x</sup> Pag. 33.

<sup>y</sup> Ibid.

<sup>z</sup> Pag. 34.



hanged, it seems. Yet, let Mr. *Robins* once in his life shew a little ingenuity; let him acknowledge any one of the oversights, to use no worse term, which his *Full Confutation* is so very full of, not less than one hundred in number, and he shall see I will treat him with more humanity.

For instance, he being now enabled to comprehend my 9th proposition, and having been taught that a ray, which meets a refracting surface, when it is in a disposition to be reflected by that surface, must actually be reflected, cannot but see the absurdity of the opinion he has hitherto maintained, that "at the thickness of the plate, where the white of the first order is reflected, NO PART of the light which enters it, arrives at its farther surface in a fit of easy transmission. For if this were true, all the light must be reflected at that thickness, and consequently, the white of the first order in glass or water must exceed the light of white metals, those not reflecting the whole of the incident light. Now let Mr. *Robins* recant this opinion, and I hereby promise never to upbraid him with it.

We come now to the method I have taken of accounting for the multiple appearance of long narrow objects seen by indistinct vision.

In order to this, it is shown in the *Essay*<sup>x</sup>, that the image of a lucid point, supposed to emit homogeneous light, and indistinctly seen, must, by reason of the alternate fits of transmission and reflexion of the rays, consist of luminous and dark rings alternately placed upon the retina.

To this Mr. *Robins* objects<sup>y</sup>, that the rings formed at the cornea by this means, would be so very numerous and consequently so very small, that none  
of

<sup>u</sup> Remarks, pag. 110.

<sup>y</sup> Remarks, pag. 108.

<sup>x</sup> Art. 209, &c.

of these rings can even upon my own representation of this doctrine be ever sensible. And from this he is pleased to conclude, not only that I utterly misapprehend Sir Isaac Newton's doctrine, but am also ignorant of the most obvious consequences of those absurd conceptions, I had formed to myself upon this subject.

I answered<sup>a</sup>, that what I suppose to be perceived by the eye, is not the single rings formed at the cornea, nor the single rings formed at either surface of the crystalline, but the combinations of those single rings, whereby larger rings are formed upon the retina; and for the proof of this I referred him to several articles of my *Essay*.

Now, Sir, the having committed such an oversight, and that accompanied with so much insult too, were enough, you would think, to put any man of common modesty out of countenance. But Mr. Robins, you see, far from being in the least abashed, gives me gravely to understand, that the change he made in my supposition for me, <sup>a</sup> was a circumstance altogether in my favour; for I, it seems, <sup>b</sup> suppose these rings to be visible, though under circumstances much less advantageous than those, he has mentioned. What can be more generous and candid, in appearance at least? But as this gentleman is not famed for candour, and the generosity of an enemy is always to be suspected, you will not think me unmannerly, if I look this gift-horse a little in the mouth, to see if the change be so much in my favour, as he assures me it is.

Let us admit with Mr. Robins, that when a point emitting homogeneous light is seen by indistinct vision, the image formed upon the retina would, from the refraction of the cornea alone, consist of

600

<sup>a</sup> Reply, pag. 36.

<sup>b</sup> Ibid.

<sup>a</sup> Full Confutation, pag. 35.

600 rings alternately dark and luminous: and in order to examine what change will be made in these rings by the refraction at the anterior surface of the crystalline, let us, for greater simplicity, at first consider what will happen to a single luminous ring placed between two dark ones.

If the middle part of the light, in its progress from the cornea to this luminous ring, be reflected back at the anterior surface of the crystalline, while the inner and outer part of this light is transmitted to the retina; then, instead of one, we shall have two smaller luminous rings, separated by a small dark ring lying between them. And if this should happen to every one of the lucid rings formed by the cornea; instead of 300 lucid rings, which we had before, we shall now have 600 smaller lucid rings, and as many dark ones.

And if every one of these last 600 lucid rings were to be divided in the same manner by the hinder surface of the crystalline; we should then have 1200 lucid rings still smaller than the former, and the same number of dark rings. On these suppositions therefore, the reflexion at the two surfaces of the crystalline would render these rings less perceptible, than if they were formed at the cornea alone.

But on the other hand, will it not sometimes happen, that the part of the light, which is reflected back, shall be contiguous to the dark ring, or so nearly adjoining to it, that the distance between them, though luminous, shall be utterly imperceptible? In this case will not the former dark ring be augmented in breadth by as much at least as this light now reflected, would have taken up in the adjoining luminous ring?

Also, will it not sometimes happen, that the whole of the light tending to a luminous ring

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formed



formed by the cornea, shall be reflected back by one or both surfaces of the crystalline? In this case, that luminous ring will be turned into a dark ring, and being joined to the other two dark rings on each side of it, will compose one dark ring as broad as the two former dark rings and the intermediate lucid ring put together. And if the same thing should happen to two, or three, or more of the neighbouring lucid rings, we may by this means have 5, or 7, or a greater number of the rings originally formed at the cornea, now united into one dark ring, which consequently will be large enough to be easily perceived.

An instance of an appearance something like this does so sensibly shew itself, in passing by a place railed in, that it will often strike the eye of the most incurious and inadvertent spectator.

When we view one range of rails only, both the rails and the intervals between them appear in their due proportion. But, in seeing two ranges together, one, two, or three rails of the nearer range will often be projected into the intervals between those of the farther range, and by filling up those intervals will exhibit the appearance of one rail, three, or five, or seven times as big as one of the single rails. Now this appearance is so apposite to our present purpose, that if Mr. *Robins* shall ever happen to pass along *Lincoln's Inn Fields*, or *St. Paul's Church-yard*, or to walk round *Moor-fields*, with his eyes in his head, I must beg of him to make the observation.

Between any two of these larger dark rings, which by the means above-mentioned may be formed upon the retina, the intermediate space, consisting of luminous and dark rings too small to be singly perceptible, must consequently appear as one luminous ring; and this we may easily conceive

to be of a breadth sufficient to render it perceptible.

Having shown how easy it is to conceive the formation of large rings absolutely dark, I must observe, that it is still easier to imagine, how large rings comparatively dark may be formed. To this end we need only suppose, that more of the light tending to the luminous rings is reflected back in some places than in others.

Therefore the luminous and dark rings formed at the cornea alone, being utterly imperceptible by reason of their smallness; and the larger combinations of them, which are owing to the crystalline humour, being easily perceptible; I hope Mr. Robins will not continue to assert, that *“ his supposing them to be continued without interruption from the cornea to the retina was a circumstance altogether in my favour. And when he is convinced of this mistake, it may do him good another time, by rendering his decisions less precipitate, and bringing him to some distrust of his own judgment, quod sentio quam sit exiguum, if he will calmly ponder and reflect upon his conclusion of the paragraph before us<sup>d</sup>, “ Since in fact these rings NEVER CAN be augmented in breadth at these surfaces, but would “ be always diminished, the whole of our animadversion must be confessed to remain in full force “ against him.”*

He would do well to consider likewise what reason he had to say<sup>c</sup>, “ The crystalline to produce “ the appearance in question ought to obliterate all “ (the rings) but 5, or 6, or a less number at regular distances.” I never said any thing about *regular distances*, the distances indeed being generally very irregular: and as to the number of images

E 2

occasioned

<sup>c</sup> Full Conf. pag. 35.

<sup>e</sup> Ibid. pag. 35.

<sup>d</sup> Ibid. pag. 35, 36.

occasioned by these rings, I have said in the *Essay*, art. 203. that when the experiment is tried to the best advantage, you will see lines alternately dark and luminous answering to those rings, in *greater number than you can count*. But perhaps this representation also is made *altogether in my favour*.

Mr. *Robins* <sup>f</sup> seems to think it strange, that I should say an objection *appears plausible at first sight*, and yet should be at a loss *to know certainly what the objection is*. But you know, Sir, it is no uncommon thing for a person to talk plausibly, and withal so loosely and indistinctly, that it is a very hard matter to know what he would be at. Besides, though this gentleman talked with his usual sublimity in one place, he was something more intelligible when he came to sum up my errors in his recapitulation.

The case I have laid down in article 209 of my *Essay*, was chosen as the most simple I could think of, in order to give the reader an intelligible notion of the lucid and dark rings formed by a single refracting surface: And if the light, which is there supposed to be emitted from a lucid point, be perfectly homogeneous, Mr. *Robins* admits,<sup>g</sup> that rings alternately dark and luminous will be formed in the manner we have supposed. He objects indeed, that these rings will be too small to be perceptible: but to that we have already answered, that we no where suppose these single rings to be perceived.

We intended farther by the case there laid down it should be understood, that rings alternately dark and luminous, not absolutely, but comparatively such, would be formed, when the emitted light was not *purely homogeneous*, but *principally so*.

By

<sup>f</sup> Ibid. pag. 36, 37, 38, 39, 40. <sup>g</sup> Ibid. pag. 42, 43, 44.



By saying *principally so*, or *nearly such*, both which expressions I have used <sup>h</sup>, is not, nor cannot possibly be meant such a light as that which *consists only of the 50th part of the contiguous species of yellow*, as Mr. *Robins* would have the reader think. The light he speaks of, consists of, perhaps some millions of different species, and contains, as far as we know, no more simple rays of any one of those species, than of any other. This cannot therefore justly be stiled *nearly homogeneous*, or *principally homogeneous*. A light *nearly*, or *principally homogeneous*, must consist for the most part of simple rays of some one species, though others may be intermingled with them: And taking my expression in this sense, the compliments Mr. *Robins* makes me upon my invincible ignorance <sup>k</sup>, *after the most elaborate attention*, may be more properly applied somewhere else.

But this gentleman will by no means allow, that in my 209th article, the light is understood to be homogeneous. His argument is, that I have <sup>l</sup> *applied to the mixed light of common objects those deductions, which are only true of light the most rigorously homogeneous*. Here I ask, how was this application made? Have I formed any proposition about compound light, and demonstrated that proposition by means of the deductions from that article? This Mr. *Robins* will not pretend to affirm. All he can affirm is, that after particularly considering this one simple case where the light is homogeneous, and falling upon one refracting surface only, exhibits the appearance of rings alternately dark and luminous, I have asserted, but without offering any demonstration, that like rings will be formed, where

E 3

there

<sup>h</sup> Reply, pag. 41, 42, 43.<sup>l</sup> Full Conf. pag. 45.<sup>k</sup> Ibid.<sup>l</sup> Ibid. pag. 42, 43.

there are three refracting surfaces, and where the light is compound; which is to be understood, *mutatis mutandis*.

Now this I own I have done, and in my *Reply*, pag. 41. I acknowledge an omission occasioned by the haste in which the *Essay* was drawn up. I have there confessed, that “ it would have been proper “ to have applied this doctrine in the first place to “ phænomena, in which the light is homogeneous, “ or at least principally so; after which, and not “ before, I should have proceeded to the phænomena produced by compound light.”

But neither confession nor excuse is of the least avail: I might as well have offered them to *Rhadamanthus* himself. This inexorable censor will by no means admit of so <sup>m</sup> *trifling an attempt to palliate my errors*. “ He well knows, says he, that our “ censure is not founded on what he has omitted “ to do, but on what he has actually done: we do “ not accuse him of having proceeded to the consideration of objects seen by compound light, before he had considered those seen by homogeneous light, but we accuse him of having ignorantly “ applied to the compound light of all visible objects those conclusions, which are only true of light absolutely homogeneous, &c.”

Amidst all this severity, it is some comfort however, that my *conclusions are true* in the case of *light absolutely homogeneous*: They must therefore be nearly true, when the light is principally, or nearly, or mostly homogeneous. And they must also be near the truth, when any considerable part of the light, though not the greatest, is homogeneous, and the rest of the light is divided into many other species. For then these other species having different intervals between their fits, and consequently being

being always partly reflected and partly transmitted, that one species, of which a considerable part of the light consists, will always predominate, and will exhibit the same appearances nearly, as if the light were all of that one species. The same thing will also happen, if this predominant part of the light be not of one species only, but be divided among two or three different species, provided the proportion between the intervals of their fits be expressible in small numbers.

Now let us suppose for a moment, that I had not been guilty of the omission spoken of above, but after treating of light absolutely homogeneous, had proceeded to the consideration of light principally, or nearly, or mostly homogeneous, or so far homogeneous as that one species predominates over all the rest. If I had shown, that the phænomena of long narrow objects seen by such a light, were exactly such as ought to arise from the alternate fits of transmission and reflexion; and had also shown that these phænomena were perfectly analogous to those I had recited from Monsieur *De la Hire* and my own observation, where the objects were seen by common light: might I not then justly have inferred, that the phænomena in the one case proceeded from the same cause, as in the other? If Sir *Isaac Newton's* authority be of any weight with Mr. *Robins*, I may here alledge for my justification the second rule of philosophising laid down by that Great Man. *Effectuum naturalium ejusdem generis eadem sunt causæ.*

Were it necessary to say any thing farther upon this occasion, I might acquaint you, that when I first engaged in the consideration of distinct and indistinct vision, I had no thought of Monsieur *De la Hire's* problem. It was mentioned to me by a learned friend, when I was at work upon my



*Essay*; and I was induced to consider it not only by the singularity of the phænomenon, but by the great reputation of the author, who I easily saw had failed in the solution, and by no body's having attempted it since his time. But though I thought the problem worth considering, I soon found into how great a length it was like to carry me, had it been accurately treated of in the case of compound light; and therefore contented myself with giving my reader a general notion of the cause it depended on, by the simpler case of light purely homogeneous.

However, if what I at first wrote upon this subject, was deficient, I have now supplied it: if erroneous, I have since endeavoured to rectify it. On the occasion of Mr. *Robins's Remarks* I have now taken into consideration the case of compound light: but far from mending the matter, I have thereby, it seems, greatly increased his <sup>a</sup> *surprise at my incorrigible mistakes*. He “wonders, how one, who but just before had so signally experienced his proneness to error, should yet without increasing either his knowledge or his diffidence attempt a second time the investigation of a subject, that had once so unhappily misled him.” “It <sup>b</sup> were paying,” he says, “too little deference to the judgment of the reader particularly to expose the numerous fallacies, and ignorant suppositions, with which this new modelled scheme abounds in almost every paragraph. I shall therefore content myself with animadverting on three capital errors, either” (any one) “of which being confuted, the whole scheme falls to the ground.”

Now,

<sup>a</sup> Full Confutation, pag. 48.

<sup>b</sup> Ibid. pag. 49.

Now, Sir, if I shall plainly shew you, that the *three capital errors* here imputed to me, one I suppose by each of the *Triumviri*, are in reality mistakes of their own, you will easily judge what credit is to be given to this gentleman, in relation to the *numerous fallacies and ignorant suppositions*, he fancies he sees in almost every paragraph.

The first of these capital errors is, my “weak  
“ conception of a pencil consisting of a whole colour, or at least of the greatest part of it, being  
“ to be found altogether in fits of transmission or  
“ altogether in fits of reflection after passing  
“ through much greater intervals” *than one fiftieth part of an inch.*

Now to this my answer is very short: I have no such *conception*. Throughout my <sup>d</sup> explanation I never once speak of *a whole colour*, or of *the greatest part* of a whole colour: I speak of simple rays only. Where I name a colour, as red, or violet, it is not the whole colour, but the extreme part of that colour: and this I call sometimes *homogeneous light of the extreme red*, or of *the extreme violet*, and sometimes *simple rays of the extreme red*, or of *the extreme violet*, on purpose to prevent mistakes, if it were possible by any precaution to prevent mistakes in Mr. Robins.

The second capital error is as follows. “ Dr.  
“ Jurin agrees, that the appearance here endeavoured to be accounted for only takes place in  
“ indistinct vision, and undertakes to shew, that  
“ the dark and bright rings, to which he supposes  
“ it to be owing, *will be formed upon the retina,*  
“ *when the luminous point, we look at, is out of the*  
“ *limits of distinct vision*, and yet there is no circumstance

\* Ibid.

† Reply, pag. 49, &c.

• Full Confutation, pag. 49.

“ circumstance in his solution, but what equally occurs, when the vision is absolutely distinct. Nay the magnitude of the interval  $Gg$ , (on which it should seem, the breadth of these supposititious rings ought to depend) may be much greater in distinct vision than in indistinct vision.”

Now in *vision absolutely distinct* Mr. Robins and I are both agreed, that all the rays issuing from one point of the object emitting compound light, are united in one point upon the retina. What we differ in is this. I, who *have learned the principles of Opticks from my friend's book*, am of opinion, that when the simple red and simple violet rays proceeding from the same point of the object, fall upon different points of the retina, as  $g$  and  $G$ , they do not both fall upon the same point. But Mr. Robins, who has studied under a master of greater penetration, has discovered, that when these two sorts of rays do both fall upon the same point of the retina, *the magnitude of the interval between them may be much greater than* when they fall upon different points. Surely one of us must be in a *capital error*.

My third error appears to Mr. Robins more *wonderful* than either of the former. It is, that I  
 “ should endeavour to account for an appearance which is no ways tinged with various colours, on principles, which, if they were true, would diversify that appearance with as many colours as the rainbow.” “ This procedure,” says he, “ is the more inexcusable in him, as I had expressly informed him in my Remarks, that, if this phenomenon did really depend on the fits of transmission and reflection, as he supposed, then instead of consisting of dark and lucid parts only,



“ ly, as it in fact does, it ought of necessity to  
 “ appear coloured; to which he has made no  
 “ reply.”

Here, however *inexcusable this procedure* of mine may appear to Mr. *Robins*, I must beg leave to offer the following excuse. He did indeed assert in his *Remarks*, pag. 107. but as he gave no proof of that assertion, I cannot say he *informed* me, that upon my principles, *instead of a multitude* (more therefore than 5 or 6 or a less number) of *separate images a broad one should have presented it self variegated with colours*. But I, who have learned to repose no great confidence in his assertions, even in matters of fact, much less in points of science, looked upon this as a mere *gratis dictum*, and therefore *made no particular reply* to it.

But when I came to lay down my solution of the appearance in the case of compound light, I shewed how part of the simple rays of every colour, and of every degree of those colours, that is, part of the simple rays of each of the innumerable species that compound a ray of white light, might fall upon both the luminous and the dark spot, G and g, the manifest consequence of which to any body but Mr. *Robins* and his two assistants, is that one of these spots must appear whitish, and the other darkish, without the least appearance of colour.

Having now shown how little my solution is affected by these *three capital errors*, capital indeed, but none of mine, I hope I may *spare the labour of framing a new hypothesis*. I shall however examine into those <sup>b</sup> *particulars relating to the multiple appearance of objects seen indistinctly*, which this gentleman so kindly informs me of, to prevent the bewildering  
 my

<sup>a</sup> Reply, pag. 50.

<sup>b</sup> Full Confutation, pag. 51.

*my self farther in this subject.* These, he says,  
 “ will not only evince, that the fits of transmission  
 “ and reflection have nothing to do in this affair,  
 “ but will also prove, that the cause, I had hinted  
 “ at in my Remarks, is undoubtedly the genuine one.”

The first particular I am here informed of, is what I had informed this gentleman of long ago in my *Essay*, that a narrow line of light between the edges of a parallel ruler, when seen out of the limits of distinct vision, will exhibit the appearance of many luminous and dark lines alternately. Upon which he says, “ If Dr. *Jurin*’s assignment  
 “ of the cause of this appearance can at all take  
 “ place, the stars ought not to appear radiated,  
 “ but invironed with concentrick circles of light,  
 “ as the forementioned line of light is bordered  
 “ with parallel lines.”

To this I answer, that in my *Essay*, art. 222. I have shown, that by means of the involuntary and imperceptible nutations of the eye, in looking at a star, the continuity of the concentrick rings will be broken; and that, if these nutations do swiftly succeed one another, the light will seem to project out different ways at the same time, which will occasion what is called the radiation of a star.

But a star is to be considered as one lucid point, the appearance of which is altered upon the least motion of the eye: whereas in the line of light between the edges of the ruler, the appearance is owing to a great number of lucid points, all of which can hardly alter just in the same manner at the same time, and if they did, could only occasion an imperceptible change in the degree of light, and in the situation of the luminous and dark rings, so that here the appearance must be infinitely more steady than in looking at a single lucid point.

The

The second particular is, that a very small pin hole in a piece of paper held against the light, nearer to the eye than to be seen distinctly, will appear radiated like a star, not encompassed with luminous circles.

But here, not to insist upon the irregularity of this hole, no pin being perfectly cylindrical, and least of all so towards the point where it is filed, which part alone must be used to make the hole *very small*; nor that the edge of the paper is irregularly torn and jagged by the passage of the pin; nor that the jagged part stands up like part of a concave pyramid, or cylinder, upon the rest of the paper, nor upon the small ridges and furrows in the make of the paper; nor upon a multitude of other circumstances, that may greatly affect the appearance; I shall only answer, that the smallest pinhole is much too broad an object to pass for a single lucid point, especially when held nearer to the eye than the inmost limit of distinct vision, as is directed by this circumspect observer.

As to the lucid specks in most, if not all the rays issuing from the pin-hole, I can only say, that till they have been carefully observed, and their situation well described, and their appearance plainly accounted for, I can make no judgment, whether they are any way analogous to the lines of light seen between the edges of the parallel ruler. These last are sometimes very numerous: whether the specks are so or no, Mr. *Robins* does not say, and for my self I could never satisfy my self about them.

My solution therefore does not appear to be in the least affected by these particulars; and as for the proof we had been promised, *that the cause*  
*hinted*



*binted at in this gentleman's Remarks, namely, the corrugation of the eye, is undoubtedly the genuine one, I cannot after the most diligent search throughout this paragraph, though the proper place, find a single word said about it. 'Tis a great disappointment, but may be the better borne, forasmuch as I have most irrefragably proved in pag. 21. that this corrugation is an empty surmise, and has nothing to do with the appearances.*

It is now, Sir, high time to conclude this tedious letter, the length of which I hope you will the more easily pardon, because though I have been wastful of your time, yet, you see, I have been extremely careful of your reputation. As great an honour as your name would have been to my title page, I have wholly debarred my self of it, not making use of so much as the initial letters, or any other mark of distinction, except only your title of Esquire, for fear of setting you up as a mark for the resentment of angry men. I should hardly have ventured upon that title it self, but that the Esquires of *Great Britain* are too numerous to have but one neck, and are besides the masters of so many votes, that upon an approaching election, there is no danger of your being wounded through their sides. If you will be as careful on your part, and be sure never to speak a word in my commendation, you are safe enough: but if otherwise, you see by this gentleman's *Preface*, what you are to expect. In a moment you will be stripped of all your good qualities and accomplishments, natural and acquired, you will have left you neither judgment nor learning, nor taste, nor good sense, nor Geometry, nor Philosophy; the coffee-houses and the press will ring of your *gross and matchless ignorance*, in a word, you will find your self in the same deplorable

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plorable condition with *Euler* and *Bernoulli*, *De Buffon* and *Dr. Smith*, *Philaethes* and my self. I am, with great Esteem and Respect,

S I R,

*Your most obedient*

*humble Servant,*

Austin Fryars,  
November  
18, 1740.

JAMES JURIN.







